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THE TEMPORAL MAZE AND KINAESTHETIC SENSORY PROCESSES IN THE WHITE RAT

WALTER S. HUNTER

The University of Kansas

In spite of the generally conceded fact that kinaesthetic processes are of fundamental importance in animal behavior, almost nothing is known that bears specifically upon these processes. All motor responses involve them, and maze studies have particularly emphasized them. In addition scattered observations abound in the literature describing position habits which have interfered with work directed primarily toward the analysis of other forms of sensitivity. In some cases these position habits have been merely the tendency to go to a definite side of the discrimination box at each trial. In other instances tendencies to alternate from one side of the apparatus to the other have been observed; and at least in one series of experimentation (dealing with audition in the rat) this simple alternation has been complicated by the addition of a sensory complex accompanying success. In this case the rat would go to one side of the apparatus trial after trial until escape was possible, whereupon it would go to the other side until escape was possible there. This was a case of simple alternation after success. It could not be termed a purely kinaesthetic automatism because of its modification by the factor of success. Success meant a free passage; failure meant running into the end-stop of the apparatus (1).

Except for Carr's recent extended study of simple alternation (2) no attempt has been made to disentangle the kinaesthetic processes incident to all studies of discrimination from the matrix of other sensory processes in which they are obscured. The present study, first reported in 1918 (3), attempts to determine how much a rat can do in terms of kinaesthesis using the following problems: simple alternation; double alternation (twice to

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the right side of the apparatus, twice to the left side, etc.); and a problem termed the "temporal maze," to be described below. Kinaesthesis may be the fundamental sensory process for animals, and still the animals may be able to do very little with it. Such a study as the present will aid in the solution of this problem.

Problem 1. The acquisition of a habit of simple alternation. In this problem seven rats were used, each approximately four months old. All were untrained in previous problems. The apparatus was the T-shaped discrimination box of figure 1. Punishment (electric shocks) and reward were used. As a pre-

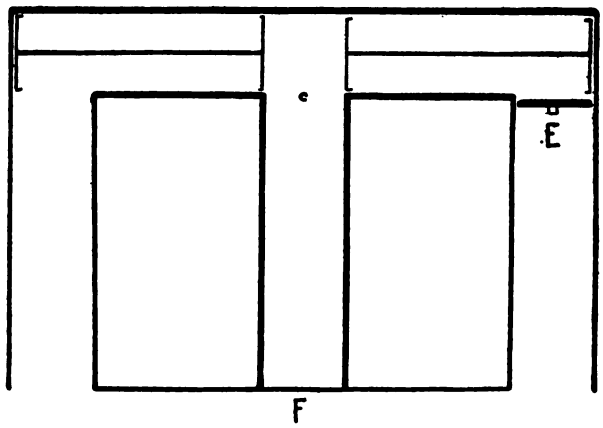

FIG. 1. T-SHAPED DISCRIMINATION BOX

E is the end-stop; *F*, the feeding place; *c*, the point of choice for right or left turns. The extent of the electric grills is shown by the brackets in the upper alley.

liminary, each rat was given the run of the box for two days prior to the beginning of the regular test. During this initial period, no end-stops were used and no food was given until the animal was taken out for the day. In the regular tests the end-stop was shifted so that the rat was forced at one trial to run to the left in order to secure food and avoid punishment and at the next trial to run to the right for the same purpose. The rat was fed and the end-stop was shifted between trials. Ten trials were given daily in the order *lrlrlrlrlr*. No external stimuli of light, sound, or odor were present to guide the animal. The amount of time consumed between each trial was about twelve seconds.

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Table 1 indicates the number of trials required by each rat to master the series of alternate runs. The criterion of mastery was an average of 87.5 per cent correct for four days, with no day's record below 80 per cent. The data indicate that the simple alternation habit is acquired in from one to eighteen days, under the conditions of the present experiment. No explanation in terms of external causes is available for the great variation in ability displayed by the animals.

After the mastery of the problem, the end-stop was no longer used. No confusion of the animals' responses was produced. Undoubtedly the stimulus for turning in one direction, let us say to the left, was the complex of kinaesthetic, organic, cutaneous,

TABLE 1

Number of trials prior to mastery of simple alternation

RAT	NUMBER OF TRIALS
30	100
31	80
33	60
34	50
35	180
36	100
37	10

and olfactory processes involved in a run through the opposite side of the box and terminating in the securing of food.

Problem 2. The acquisition of a habit of double alternation. Using the same apparatus as before, with punishment and reward, tests were made upon the ability of rats to master a double alternation, *llrrllrrll*. After the preliminary acquaintance with the apparatus, the rat was inserted with the end-stop in such a position as to force it through the left side of the apparatus. It was then fed and re-inserted and forced to come through the same side of the apparatus in order to escape. The next two trials were through the right side of the box.

To the great surprise of the experimenter, this apparently simple problem was never mastered by any one of nineteen rats who were tested on it under various conditions. The details of

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these abortive attempts at learning and the conditions under which they were made are as follows.

Rats 30, 33, 34 and 35 of the previous problem, simple alternation, were each tested on double alternation for 550 trials, ten trials daily. An interval of twelve seconds between each trial was consumed in feeding and re-inserting the rat. None of the rats made any progress toward the mastery of the problem during these fifty-five days.

Rats 5, 6, 7, 8, 16, and 18, untrained rats two months old, were trained on the problem of double alternation, under the above conditions. Rat 18 received 464 trials. The other received between 600 and 650 trials. At the conclusion of the training period, no rat had made significant progress toward mastery of the problem.

Rats 5, 6 and 16 were now tested on the same problem under slightly different conditions. The end-stop was placed, not in the side alley as indicated in the figure, but just beside the point of choice (*c*) so that the animals were forced to run in double alternation without the possibility of error. Each rat was given 400 trials under these conditions. At the close of this period, the end-stop was placed as usual in the side alley. Twenty trials were now given each rat with the possibility of choice again present. No rat made better than 60 per cent of correct reactions.

In the tests so far described in this section, failure may have been caused either by the length of time interval between trials or by the fact that in the series of ten trials six were to the left and only four to the right. In order to test this, three new rats (51, 52, 53) were tested on double alternation giving eight trials daily, *llrrllrr*. Punishment was used, but food was only given at the close of the day's work. As soon as a rat ran to the exit, he was immediately re-inserted for another trial. There was thus practically no interval between trials. Rats 51 and 53 received 512 trials. Rat 52 received 488 trials. At the close of these tests, no rat had made significant progress toward mastery.

Rats 2, 3, 4, 100, and 101, untrained and two months old, were tested on this problem with four trials daily, *llrr*. There was an

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interval of twelve seconds between each trial. Punishment and reward were used. All were given 200 trials, 50 days' work. At the close of this period no rat had made significant progress toward mastery. (Throughout the description of these experiments the phrase "no progress toward mastery" means that there was no evidence of improvement to justify the assumption that if the tests were long continued learning would be completed.)

Problem 3. The double alternation "temporal maze." Following this series of failures to set up the double alternation habit above described, an entirely new method was attempted. Alternating behavior, whether it be simple, double, or more complicated, is analogous to the running of a maze where the response is interrupted from point to point and food given. Might it not be if the rats were trained in a maze where the choices were arranged in an *llrrllrrll* manner that, when transferred to the T-box, they would be able to learn the double alternation problem? In the first test we should have established a double alternation habit in a conventional maze, and in the second test we should be looking for a transfer of this kinaesthetic automatism to the new conditions. It is true that if this transfer did not take place, one could not conclude that the animals could not form the double alternation habit, for there would be many novel conditions that would work against a transfer. However, having failed by the direct method of approach, the indirect method was worth a trial. In order to make the T-box test more comparable with the maze, it was converted into a *temporal maze* as shown in figure 2. The conventional maze is termed by way of contrast a *spatial maze* and is shown in figure 3. A further word of description of each of these mazes is necessary.

The temporal maze was manipulated as follows: With the entrance-stop as indicated in figure 2 and the end-stop on the right, the animal was placed at *E*. When in the course of its explorations, the animal came down the side alley to about the point *L*, the entrance-stop was shifted to the dotted position on the right. This left a circular path, with the one off-shoot to the right, through which the rat might trace and retrace. After the rat had made its second trip around the left side of the ap-

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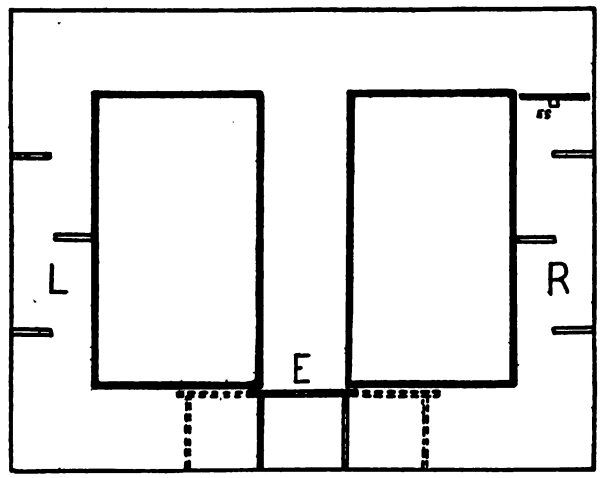

FIG. 2. THE TEMPORAL MAZE

E is the entrance-stop which can be shifted right or left as indicated by the dotted lines; *es*, the end-stop; *L* and *R*, points referred to in the paper. The three partitions in each of the side alleys were present in only one control test with rat 110. They were present in no other test.

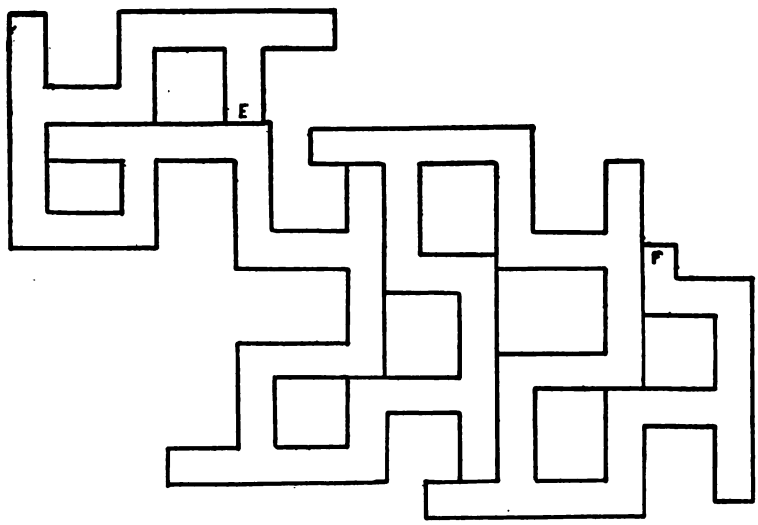

FIG. 3. A SPATIAL MAZE COMPOSED OF SUCCESSIVE T-SHAPED UNITS WHERE THE CHOICES ALTERNATE AS *llrrllrrll*

E is the entrance, and *F* the food-box.

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paratus and reached the point *E*, the end-stop was shifted to the left alley (dotted position) and the entrance-stop was again shifted to its central position. When the rat had run through the right alley and reached about the point *R*, the entrance stop was shifted to the dotted position on the left. It remained in this position, until the rat was ready for two trials to the left again. This method was followed until the rat had been forced to run *llrrllrrll*, i.e., the regular ten trials. It should now be clear why this problem is termed a temporal maze. The rat is compelled to run through the *same* pathway in varying combination during *successive* moments of time. In the spatial maze not only new moments of time are involved but also new portions of space. Or to put the situation differently, in the spatial maze the cues are distributed in new segments of space as well as in new moments of time; while in the temporal maze, the cues must arise in the same space during the ten trials, but will succeed each other as a temporal sequence. The rat has demonstrated his ability to master almost every degree of complexity in a spatial maze. What are the limits of his ability in a temporal one? The shifting of the end-stops was not noticed by the animals. It is possible that at times the shifting of the entrance-stop was noticed and thus served to distract the animals. No punishment was used and the animals were fed only at the close of the day's test.

The spatial maze illustrated in figure 3 was constructed in order to present ten points of choice for the rat when running forward along the true pathway. The correct choices were arranged in the sequence *llrrllrrll*. The maze was uncovered and painted black, with alleys 8 inches deep. The T-shaped units of this maze were arranged in series as is necessary in the conventional maze. Therefore in addition to the qualitative cues which arise as the animal runs, there will be either spatial characteristics attaching to these cues or else special space cues conditioned by the extended nature of the pathway. As contrasted with this, the temporal maze has but one unit, and the space cues are eliminated.

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Six untrained rats, nos. 102, 103, 104, 105, 106, 107, two months old, were trained on the space maze of figure 3. One trial per day was given (equivalent to ten trials in the T-box). The criterion of learning was a perfect run on each of three successive days, a very much higher criterion than was used with the discrimination box. Table 2 indicates the number of days used by each rat prior to the perfect runs. The table indicates that the rats acquired the association very readily. There was nothing in their learning curves to mark this maze as peculiar.

TABLE 2

Number of trials prior to three perfect runs in the spatial maze

RAT	NUMBER OF TRIALS
102	5
103	8
104	10
105	10
106	12
107	6

TABLE 3

Number of trials in temporal maze

RAT	NUMBER OF TRIALS
102	28
103	43
104	46
105	43
106	42
107	42

The six rats trained on the spatial maze were now transferred to the temporal maze using the method described above. One trial per day was given. Table 3 indicates the total number of trials given each rat. At the close of these periods, no rat had mastered the problem. All had fallen into position habits which made the continuation of the test useless. During the progress of the training improvements took place in the following particulars: (1) the tendency to retrace or run through the maze in the reverse direction was practically eliminated; (2) the time was

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decreased from an average of eighteen minutes (m.v. 3.5 minutes) on the first trial to an average of three minutes (m.v. 35 seconds) on the last trial.

During the training the following tendencies and characteristics of behavior were manifested: a tendency toward simple alternation; a tendency to run a loop of the maze in the reverse direction as though continued activity of this type might bring the final reward of food; a tendency to alternate after success, as described at the first of the paper; and finally a tendency toward greater variability in the first four choices than in the last six constituting the day's test. Of these the tendency toward simple alternation and the tendency to alternate after success

TABLE 4

Behavior in temporal maze

RAT	SERIES OF CHOICES				
	ll	rr	ll	rr	ll
103	rw	rw	rr	rw	rr
	rr	ww	rr	ww	rr
	rw	ww	rr	ww	rr
	rw	ww	rr	ww	rr
104	wr	ww	rr	ww	rr
	wr	ww	rr	ww	rr
	rw	ww	rr	ww	rr
	ww	ww	rr	ww	rr

have appeared so constantly in all difficult problems with the T-box, that it seems almost safe to affirm that these are innate reaction tendencies of the rat.

In illustration of the last of the four mentioned behavior tendencies, the series of choices made by two typical rats on four successive days at the close of the experimentation can be indicated in table 4. *W* indicates a wrong choice. *R* indicates a correct choice. In each case the reference is to a choice occurring at the point where the right and left paths diverge at the top of the T. An inspection of this table will show that the first four responses are not identical from day to day. The last six responses indicate a position habit of going to the left. Alterna-

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tion after each successful discovery of a free passage gives a record of this type, selected from the data for rat 106, *rwrwrwrwrw*. Simple alternation is illustrated by this sample behavior from rat 102, *rwwrrwwrrw*. Here the animal ran first to the left and alternated regularly thereafter, giving the record just quoted.

In case an animal fell into an automatism which repeated itself from day to day without essential variation, one would have a temporal maze habit, although not the particular one which was sought. Rat 105, e.g., was the one rat who acquired such an invariable form of response. This rat always alternated after each success for the first four choices and then ran a position habit to the left for the last six choices. His record therefore day after day was *rwrwrrwwrr*. Here was an automatic chain of unit responses which involved running through the same space in succeeding intervals of time. What were the cues involved? Was the cue a kinaesthetic one derived from turning to the left or right, or was it a combination cue involving a contact-kinaesthetic experience with the end-stop? Was the behavior controlled essentially by cues from within the organism or from cues that depended upon specific phases of the external environment? Controls were now introduced with several of the animals that approximated this automatic behavior in an effort to answer the question. It will be possible to follow through the records for three of the animals, and then to state in general what the result has been.

Rat 105, as stated, had an invariable form of response in the order *lrrlrllllll*. A control was now introduced with this rat where all the choices were to the left. The entrance-stop was shifted after the first trial but not thereafter. The end-stop remained on the right side continually. The rat ran *lrrlrrrlrr*. The tendency to run to the right to secure food, being checked by the end-stop, overcame the normal tendency to run all of the last trials to the left, so that the left position habit appears only in choices 4 and 8. The second control used required the rat to run a series of choices five of which were to the left and five to the right, *lllllrrrrr*. Entrance- and end-stops were shifted in the middle of the series. The rat chose in the order *lrrrllllll*. In

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other words, the animal first turned to the left, then alternated to the right as usual. Failing to receive the sensory complex associated with success, he ran one more trial to the right than usual. The impulse to run to the left now asserted itself, and the remainder of the runs were made to that side. The next day the same control was used and the rat ran *lrrlrllll*. Here the interference of the reaction tendencies peculiar to the first and last halves of the normal reaction comes in the fourth and fifth trials. The tendency to run to the right until success was encountered was so strong that after the fourth choice was made the rat retraced his steps and investigated the end-stop on the right. He went again on the following trial to the right side, and only then did the turn-left tendency predominate. This same type of behavior was repeated when the series of choices was changed to *lllllrrr*. When a series composed wholly of rights was used, the animal happened to go to the right on the first trial whereupon he went left for the rest of the series. As a final test the rat was inserted without either entrance- or end-stop. It was thus possible for the animal to run in any direction. If the end- and entrance-stops were not serving as partial cues, the kinaesthetic tendencies should unroll in the normal manner. The result of the control was for the rat to make all of its choices to the left with a great deal of retracing in a clockwise direction, due apparently to the absence of the entrance-stop.

These controls indicate that the normal integration of the two reaction tendencies of rat 105 depended upon sensory complexes from outside the animal's body. The impulses for running right and left, undoubtedly carried in proprio-ceptive terms, depended for their normal functioning upon extero-ceptive data.

Rat 106 also had the tendency to alternate after each success in the first of the series, although the total series was never automatized as with rat 105. Controls were used where the series of choices in place of being double alternation were *lllllrrrrr* and *lllllllrrr*. This rat always ran to the left the first trial, then to the right until successful and then to the left, thereby alternating after each success. The sensory complex from running against the end-stop (failure) produced a return to the same side until

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the complex was absent, whereupon the rat ran to the other side. The securing of food between trials was not necessary in order to produce the alternation. It was not the kinaesthetic complex involved at the point of choice (at the top of the T) which determined the direction of the subsequent choice, but the sensory complex at the end of the alley underlying a free or an obstructed passage.

Normally rat 104 behaved in the manner described in table 4, running all to the left with the possible exception of the first one or two choices. When tested with *lllllrrrrr*, the rat responded *rrlrrrllll*, showing the tendency to alternate after success conflicting at random with the position habit to the left. With the series composed entirely of choices to the right, the animal made the first choice to the right and all others to the left.

We shall attempt later in the paper to explain why the rats could not learn the temporal maze where the demand was for double alternation through a continuous pathway.

Problem 4. The simple alternation "temporal maze." We come now to the experiments upon the temporal maze with simple alternation. Here the apparatus and method described above on pages 5–7 were used, except that the entrance- and end-stops were shifted to require a simple alternation. The rat was thus forced to run a continuous figure 8, once around the left of the box and then once around the right, continued for ten choices.

Seven untrained rats were used, nos. 108–114. Rats 108 and 109 were three months old when the tests began, and the others were five weeks old. The detailed presentation of the records is unnecessary, because they merely repeat what has already been given for the double alternation temporal maze. Only one animal, no. 110, mastered the problem. Table 5 shows the number of trials given each rat. Each trial is equivalent to ten trials in the T-box. Attention should be directed to the tremendous variation in difficulty existing between the simple alternation problem in the T-box and in the temporal maze.

In the course of the training, all rats acquired a position habit to the right. This was uncomplicated by other reaction tendencies as had been the case in the training on the previous tem-

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poral maze. Various expedients had to be used in an effort to break up this stereotyped form of response. The rats were forced constantly through the left side of the maze; or they were taken out of the apparatus and fed between each of the ten choices; or they were given several days rest. These methods succeeded only with rat 110, and this animal mastered the problem in the manner to be described below.

The animals who succeeded only in building up a right position habit were tested with the end-stop so arranged that an open pathway existed only around the right side of the apparatus. When this was done the rats either ran all of the ten choices to the right in conformity with their position habit, or once or twice made left turns. So when the apparatus was arranged to per-

TABLE 5

Number of trials given on temporal maze, simple alternation

RAT	NUMBER OF TRIALS
108	20
109	18
110	59 (mastered)
111	60
112	65
113	51
114	64

mit only free runs to the left, the animals made either all wrong choices or occasionally interspersed a turn to the left. The evidence indicates, therefore, that this position habit is practically uninfluenced by entrance- and end-stops. The behavior is undoubtedly a chain of reflexes whose stimuli are internal. This is further supported by the fact that when the rats were inserted with end-stop and entrance-stop removed so that a continuous path was open to the animal, each ran at least 80 per cent of its choices around the right side of the box. Here where no stops were used, there was much retracing about the box, but each time the rat ran through the central alley in the normal direction, it turned to the right. Reliance was therefore placed upon the entrance-stop as a cue for guiding the animal into the central

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alley, thus preventing retracing; but once the rat went through this alley, its choices were in accordance with its one kinaesthetic tendency, to turn to the right. In several cases some evidence was found indicating the rôle of the end-stop. Let us take the case of a rat who ran twice to the right meeting failure at the first and success at the second choice. This success, before the position habit was thoroughly established, would lead the animal to run to the left. It was found that if success (a free pathway) and not failure (a blocked pathway) were offered the rat on the first trial, that he would at the next trial go to the left.

We may now turn to a brief description of the behavior of rat 110 who learned to run the simple alternation temporal maze. This rat, as stated above, also developed position habits which required breaking by all three of the methods there described. The process of learning was long and tortuous. At the close of the 59 trials, rat 110 could run the continuous path of *lrlrlrlrlr* without error, save that it usually retraced at least once per trial. This retracing was usually running from some point in the side alleys back along the top of the T to the end-stop of the opposite side. In a space maze, this would probably be counted as two errors. The fastest time made by this animal was 52 seconds. Without apparent cause, the time fluctuated between this and 105 seconds. The response never became so automatic and stable as is customary in a space maze. After the habit was mastered, only the entrance-stop was used regularly. The end-stop was quickly and quietly inserted only when the rat made a wrong choice. It does not seem probable that the lack of stability in the habit was due to distracting influences. The cause of the instability was rather the difficultness of the task.

The following results were secured with controls used with rat 110: (1) When twenty continuous choices per day were given as opposed to ten, no disturbance was apparent. This control doubled the length of the maze. The animal practically continued to run successive figure 8's until removed from the maze. (2) When the entrance-stop was taken out of the maze and not used after the first trial, the rat failed to turn into the central alley. (3) The maze was rotated 180 degrees and the rat was

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inserted at the same absolute location in the room, i.e., it was inserted at what had before been the point of choice at the top of the T. The entrance-stop was used at the new extrance. Except for this all cues from within the maze were now in new locations. The external environment, the entrance-stop, and the kinaesthetic relations remained relatively unchanged. The control did not disturb the rat. (4) Obstructions were now placed in the alleys as indicated in figure 2. These were partitions extending one-half way across the alley. They forced the rat to run in a serpentine path and thus varied the general nature of the kinaesthesis. The animal's habit broke down completely in the first trial as soon as it encountered the first obstruction. This was undoubtedly due to distraction from a novel stimulus. When tested again on the second and fourth succeeding days, the rat ran normally with this control.

When the rat was inserted in the apparatus under standard conditions, the entrance-stop was in place across the entrance. The animal turned, "nosed" the stop, and then ran. It was thought, therefore, that the entrance-stop might give the initial cue to the habit. (Control 2, as well as the observation just presented, indicates the control which this stop exercises during the running of the maze.) No test was made of this possibility while the rat was making perfect reactions, but presumably the results now to be described would have been secured had the test been made at that time. Toward the close of the controls the rat developed a habit of going always to the right on the first two choices. The remaining eight choices then alternated perfectly. It was decided to place the entrance-stop across the entrance (in the position occupied on the first trial) at various times during the day's work. When the animal was ready to run through the central alley, the entrance-stop was to be shifted across the entrance thus causing the rat to turn and "nose" the stop before continuing the run. On the first day, this test was used whenever the rat would otherwise have gone to the left. In two out of five times the cue derived from the shift overcame any tendency to go left, and the rat ran to the right. On the second day, the cue forced the rat to run to the right four times out of

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five. On the third day the entrance-stop was so shifted six times, and five times the rat ran to the right when normally it would have gone to the left. Choice of the turn to the right was clearly conditioned, therefore, by the presence of the entrance-stop. This response was also aroused normally by running around the left side of the box and being forced into the central alley by the stop as indicated by control 2.

Interpretative comments. We may now comment upon the significance of the data secured on the temporal maze as they throw light upon problems connected with the spatial maze. Why can rats not learn double alternation? The answer seems to be this: The experience of running around the left side of the T (kinaesthetic, tactual, olfactory, etc.) can serve as a cue for going around the right side of the T or for going around the left side of the T again, but it cannot serve at one time for the first response and at another time for the second one. The rat can use the cue either in going constantly around one side of the apparatus or in going alternately from one side to the other. It cannot use the same cue for both responses. This clearly excludes the possibility of mastering a temporal maze where the choices might be arranged *lrlllrrlrrrl*. A given kinaesthetic complex may mean either of several responses but it cannot mean now one and now another without the addition of some selective element. The experiments have also indicated the great difficulty with which a simple alternating temporal maze is mastered. Here it was possible to set up a chain of responses each link of which was the stimulus for the next, but only one rat mastered the problem and that after prolonged coaching. In addition we have seen reason to attribute a certain indefinite amount of influence to cues derived from the entrance-stop (in certain cases even the end-stop played a rôle).

If the conventional account of the reactions in a spatial maze were correct, the rat should be able to learn a temporal maze of almost any complexity. It is said that in the former maze, the kinaesthetic cue from running one segment controls the running of the succeeding segment, so that a chain of proprio-ceptive activity results. The work on the temporal maze indicates that

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it is all but impossible to set up a mere temporal sequence of kinaesthetic processes with the rat. Running the spatial maze therefore must require cues which have space location as well as temporal position. In other words the rat must recognize in terms of space where he is in the maze. These spatially located cues may be anything but a kinaesthetic complex. This is not to say that a characteristic kinaesthesis will not arise in certain portions of the maze. It is to point out that the space location of the kinaesthesis is due to non-kinaesthetic processes (contact, e.g.). The present tests have indicated that the rat has practically no capacity to set up habits where the sensory complexes succeed each other merely in time. It is possible that a rat might learn a space maze requiring simple alternation and then run it in terms of kinaesthesis. The animal however easily masters the ordinary maze where the choices may be in any combination. How can it use the kinaesthesis connected with a left turn at one time to initiate a turn to the right and at another moment to initiate a turn to the left? Our tests have indicated that this cannot be done unless spatially arranged cues are available.

REFERENCES

(1) HUNTER, WALTER S.: The auditory sensitivity of the white rat. Jour. Animal Behav., 1914, iv, footnote p. 216.

(2) CARR, HARVEY: The alternation problem: A preliminary study. Jour. Animal Behav., 1917, vii, 365–384.

(3) HUNTER, WALTER S.: Kinaesthetic sensory processes in the white rat. Psych. Bull., 1918, xv, 36–37.

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THE BEHAVIOR OF WHITE RATS IN THE PRESENCE OF CATS

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The recently reported note[1] on the possible instinctive character of the behavior of the white rat in the presence of a cat has led to a more complete and accurate description of the facts. At the time the note was written it had been observed that the presence of a cat, or of the feline odor when carried on the hands, was a signal for the release of a striking bit of behavior in the rat. Because of the apparent unlearnedness of the responses, it was thought best to call them "instinctive." Small[2] has given the first controlled description of such activities in the white rat; but further work has by no means kept pace with descriptions of other kinds of behavior aroused during the laboratory use of these animals.[3]

The behavior previously described suggests Small's account of the "emotional response of fear" at unusual noises (p. 86). "Fear in connection with loud noise is more definitely determinable (twenty-second day); besides the reflex recoil, they crouch and huddle together and wear a subdued expression" (p. 87). Small did not find, however, that this behavior was excited by the presence of a cat or by the feline odor. "The young rats showed not the slightest symptom of fear of either man or cat, though I tested them almost daily by my own presence and by presenting my hand to their noses after having impregnated my

[1] Griffith, C. R., A possible case of instinctive behavior in the white rat. Science, 1919, l, 166–167.

[2] Small, W. S., Notes on the psychic development of the young white rat. Amer. J. of Psychol., 1889, xi, 80–100.

[3] See Donaldson, H. H., The rat, 1915, p. 28, for a bibliography of references to early accounts of the behavior of rats under natural conditions, and to later laboratory work.

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hand with cat-odor by rubbing a cat" (p. 98). He did find, however, that adult rats with no experience showed some uneasiness, which, with other facts, led him to think that "the manifestation of instinctive fear waits upon the integration and functioning of the higher centres" (p. 99).

The problem is not solved. It calls for experimental isolation, further description of the performances previously noted, and a determination of the specific relation between the responses and the presence of the cat. Several questions should then be answerable: to wit, (1) What is the chief sensory channel through which the responses are induced, i.e., what is the specific factor in a cat that should excite such behavior? (2) What relation obtains between the response and the age and sex of the rats? (3) What is the relation of this type of behavior to other well-known types of behavior?

For further work, five groups of docile white rats, varying in age and sex, and three females with litters, were used as subjects. Five cats, two dogs, a variety of odorous chemicals, and some mice and common Norway rats, were used to excite the responses. In a part of the experiments, the rats were undisturbed in their nests on an iron frame, the exciting object or substance being placed upon or in their cages after it had been brought into the room. In this way, a minimal disturbance in the normal living conditions was hoped for. In another part of the experiments, the rats were taken, singly or in pairs, to the object provided in another room. For this purpose a wire puzzle-box was used to hold the rat. Over the puzzle box was set a large glass observation-frame inside of which the cat or other object could be placed.

In the first group of experiments, an effort was made to isolate the responses and to give a complete description of them. Two female rats about five months old were placed in the puzzle-box. As is usual with normal rats, they began vigorously to explore the new environment. In all experiments of this kind care was taken to note the immediacy and the energy of this initial exploration. Within a second or two after the introduction of a cat under the large observation frame, the subjects suddenly dupli-

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cated the performances previously reported. The exploratory movements were totally inhibited. The otherwise tense position of the rats was varied by a slight trembling and a sort of spasmodic contraction in the region of the viscera. The whole attitude of the rats seemed to indicate fear. At the end of five minutes, the cat was removed. During this time the rats had not changed their tense position, the head being held up and out, the feet planted squarely on the floor, the body slightly crouching, the respiration accelerated for a few moments and then momentarily retarded.

In another series, the rats were left undisturbed in their nests, the cats being brought into the room and placed upon the cages or within them as the circumstances warranted. The behavior of the groups was still more striking than had been the case with the single rats or with the pairs. In the first trial, a cat was placed on the cage containing the five-months-old rats. Within a few seconds, activity had completely ceased in the cage and within thirty seconds all the rats, save one (described below), had retreated to one corner of the cage and were there massed together. Some of the rats were displaying the same convulsive movements of the viscera, and occasionally a low whine was detected. When the cat was placed upon the cage containing the two-months-old group, the responses were more intense than in the previous case. There was considerable nervous jumping. The huddling together would last for a few seconds, and then, as some noise or some movement of the cat occurred, the rats would run distractedly here and there and finally collect again in a corner. A mother and two young about four weeks old confirmed previous observations. The young seemed very much disturbed and did not recover for more than two hours. The mother retreated to one corner of the cage and remained there for over an hour. Another female was suckling a litter of five young, about two weeks old, when a cat was placed upon her cage. She hastily left the nest and backed to the rear of the cage with the same characteristic performance. A mother caring for a litter of nine, which were one day old, likewise hastily left the nest dragging three of four young with her. She crouched at

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the back of the cage with all the indications of fear. About fifteen minutes after the cat had been removed and efforts made to dissipate the feline odor in the room, the mother of the very young litter undertook in a hesitating and furtive way to return to the nest some of the young which had been lustily crying. She did so by seizing them in her teeth and depositing them one by one in the nest in the forward part of the cage. She frequently retreated to the rear of the cage, however, and assumed the old posture. At the end of thirty-five minutes all the young had been returned and the mother was beginning to resume her usual behavior. It was nearly an hour before the mother of the two-weeks-old litter had fully recovered. In a little over an hour the two cages of adult individuals had quite recovered, their normal feeding activities being interrupted now and then by only a slight reversion to the old attitude. These observations were further confirmed by four other cats and by repeated trials.

Now the writer has noticed that when young rats have not been handled at all until they have just opened their eyes and they are then picked up, they make violent efforts to escape. There seems here to be an unlearned response to an unusual or strange situation. The question arises, therefore, as to whether or not the responses to the cat are not usual responses to any new situation,—the newness or the strangeness itself being the essential point. The experimental procedure that has been detailed in the foregoing was, therefore, repeated with two dogs, several chemical substances, and other odors. The behavior of the dogs was quite different from that of any of the cats. There were frantic efforts to get at the rats, large amounts of saliva being secreted and dropped about the cages and the floor. The disturbance made was so great as to excite the rats, but there was nothing qualitatively characteristic about this excitement. The same sort of behavior on the part of the rats,—a running to and fro about the cage, etc.,—can be produced at any time by disturbing the nest and cage in a similar manner. Moreover, there is no after-effect, the rats resuming their normal activities as soon as the dog is removed. The use of a number of chemical odors, including ether, alcohol, carbon disulphide,

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stale cheese, syrups of various fruits, heliotrope, camphor, hydrochloric acid, nitric acid, and a variety of other volatile substances, induced much "prying curiosity," the rats following the odor as it moved back and forth below the front part of the cage. This was the case even when a cloth model of a cat was placed upon the cage in order to simulate the conditions previously established. There was no sign of fear of mice or Norway rats, nor of any part of them, the mice being attacked and in some cases eaten.

There was never anything about the behavior of the rats that would indicate movements of offense or defense. The entire inhibition of exploratory movements has already been mentioned. The behavior reminds one of nothing so much as a sort of paralysis or at least a kind of inertness, possibly the attitude an animal might take when fascinated. One four-weeks-old male was exploring the top of a nest-box when a cat was introduced. The rat immediately "froze" and hung for twenty-two minutes, one toe after another slipping loose, until finally the rat dropped to the floor where it remained on its back and side. The rats made no attempt to resist the experimenter's hand during the persistence of the odor, and, when replaced in the nest, they rested where and in the position they happened to be when set down. The time during which this behavior lasts is striking. As soon as the cats were removed from the observation-frame, the air was changed by currents as rapidly as possible; but the return of the rats to normal movements was very slow. This return, which begins within an hour after the experiment, is marked by half-hearted efforts at cleaning the face or sometimes by a very cautious exploratory movement. Attempts to attract the attention of the rats during this period were futile. The whole organism seemed set in another direction. If, however, they are handled gently for some time, the period of recovery is much shortened. We now come to the questions proposed.

1. It is evident from the foregoing descriptions that the presence of a cat is a definite occasion for the arousal in the white rat of a characteristic cessation of normal movements and activities as measured by the maintenance of a specific posture,

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and such other responses as whimpering, increased rate of respiration and heart-beat, and visceral disturbances. Just what the exciting object may be, aside from that expressed by the phrase—'presence of a cat'—cannot be stated. The sense-department involved, however, was established as follows. If a cat is placed upon a cage when the room is wholly dark and five minutes later the light is switched on, (care being taken to keep other conditions normal) a striking tableau is presented of every rat in a group huddled in some corner of the cage and giving evidence of the same behavior as above described. The visual factor, the form and movements of the cat, seems, therefore, to play no essential part. Rats placed in an enclosed space which has previously contained a cat respond as above. The response to the odor carried on the hands or in a damp cloth which has been rubbed for some time over a cat is not so sudden nor so intense but nevertheless striking. There is no response to a cat encased in a glass jar even though the cat makes a variety of movements. One rat (mentioned above) which may have been almost if not entirely anosmic from a pneumonial affection was undisturbed by the presence of the cat; for it displayed much interest in the cat's feet and body as they came in contact with the cage. Three subjects rendered anosmic by cotton stuffed in the nostrils were likewise undisturbed. It seems quite probable, therefore, that an olfactory quality of some sort is an adequate stimulus for the arousal of the behavior described. The rat does not so respond to the lungs, the heart, the blood, the intestines, the feces, the urine, a portion of the abdominal wall, or a section of the muscles of the hind leg of the cat. It probably does respond to the hair, when detached from the body; possibly to the skin, and more definitely to the nose. These portions of the dead cat did not, however, arouse unusual behavior in the rats with any amount of intensity. The temporary conclusion of investigations of this kind is that there is some olfactory quality about the living cat which sets off the responses just described. It is probably safer to describe the behavior itself than definitely to say that it is fear. The work of

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Cannon[4] and others suggest, however, that physiological investigations might furnish a more definite answer to this question.

2. Having in mind age and sex differences, the above experiments were repeated with many pairs of rats and with five different cats. Save for the litters of very young rats, the behavior was so uniform as to obviate the necessity of a detailed presentation, the responses varying chiefly in degree. A few of the subjects whined but all gave unmistakable signs of disturbance. Only five or six displayed the bodily contractions, three being females. There seems to be no sex difference save when the female is pregnant or is suckling a litter. In these cases, the reaction appears a little more marked. The younger rats, especially those two months old, were highly excitable, running nervously from one part of the box to another whenever the cat changed its position. A group three weeks old presented a comical picture. They were hardly strong enough to maintain a rigid position and their eyes were not yet accustomed to the light, having opened only the day before. Their attitude was just as characteristic and specific, however, as that of any of the adults. The performance of the group two weeks old is a little questionable. There was, of course, no bodily posture taken. The judgment had to be made entirely in terms of the amount of activity manifested in squirming about the nest seeking the mother. In the presence of the cat, these movements almost ceased, the quietness being unusual after the cat was removed; but the quiescence was in no other way striking. The day-old group made no response that could be detected even though the nose of the cat was held within 1 cm. of the rats. There was no response from the same group when a week old, nor at ten days. Watson[5] thinks that the olfactory tract is entirely unmedullated at this age or for some days later. Our results would seem to indicate that the fear-response takes place before such medullation, then, and certainly before medullation in the higher centres has progressed very far.

[4] Cannon, W. B., Bodily Changes in Pain, Hunger, Fear and Rage, 1915.
[5] Watson, J. B., Animal Education, 1903, p. 118, and passim.

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The behavior of the cats is interesting. None of them was concerned with the rats, save the youngest, which spat and struck when a rat was pushed near her. When placed in the wire cage containing the seven oldest rats, she was entirely indifferent until the anosmic female rat mentioned above essayed to explore too intimately, at which she spat viciously and struck at the rat. From this time on, the female in question evinced more of the behavior of the other rat; but at no time did she seem to be wholly afraid. Although the cats seemed to be indifferent to the presence of the rats, they all sought, captured, and eagerly ate mice which were provided. One only pursued, captured and ate a young rat.

3. We come finally to a statement of the relations this behavior has to certain other well-known kinds of behavior. It has already been made clear that there is a total inhibition of the exploratory movements. The rat seems to possess a very definite and rather well-established tendency to explore in a characteristic way every new situation in which it may find itself.[6] There has been a total inhibition of all this activity in almost every case. The one exception has been explained. In other words, the neural organization, whatever it may be, which is excited under the influence of the feline odor is prepotent over the neural organization giving rise to exploratory movements. The other tendencies to be taken advantage of are the feeding tendencies, the specific sex tendencies, and the mothering tendencies.

Repeatedly the rats have been allowed to go without food for several hours beyond their normal feeding-time. When food—cracked corn, oats, sunflower seed, and bits of cheese—was finally placed in the nests, the feeding activities were, of course, marked. On these occasions, to put a cat quietly on the cages results uniformly in the inhibition of the feeding activities and in the arousal of the behavior characteristic of the situation. When a male and a female are newly placed together in a cage the usual movements of pursuit and escape are immediately aroused. The introduction of the feline odor results, however,

[6] Small, op. cit., pp. 99 f.

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in the same way. There is an absolute cessation of activities indicative of either tendency for as long as an hour. The inhibition of the mothering activities has been indicated above. After repeated experiment, the mother of the very young no longer left the nest but crouched in a frightened manner over them. This was in no sense protective, but rather identical with the attitudinizing of the other individuals under similar circumstances. Just as striking, was the arousal of the rats from sleep. Forty-one seconds after a cat had been placed upon a cage of sleeping rats, disturbance began. The first indication was the opening of the eyes and the raising of the nostrils in an exploratory manner. Two minutes later all the rats were huddled in a corner of the cage; one was whimpering. In a few seconds, the rats in an adjoining cage were behaving in the same way.

Associations are established very rapidly with these activities. It was noticed as the experiments proceeded that they were carried over to objects originally arousing no such behavior. For some days after a two-weeks experimental session with a two-months-old group, a partial response was excited by the presence of the experimenter himself, or by objects placed upon the cages. With one group of young rats attempts had frequently been made to feed them bits of cheese during the presence of a cat. For several days after the series was over fresh cheese repeatedly excited a marked disturbance. This was especially true when the presentation of the cheese was accompanied by a clucking sound frequently made on other occasions to attract the attention of the rats. The establishment of such associations is quite illuminating in view of the work already done on "conditioned" and "unconditioned reflexes."

To conclude: the experiments here reported demonstrate that there is a specific factor, probably olfactory, about the living cat which induces in white rats of two weeks and older a marked bodily state which is suggestive of fear.[7] This state can be

[7] We have found that, in some subjects, this fear-attitude is more like Preyer's cataplexy than like the timidity or cowering observed by Yerkes (Yerkes, R. M., The heredity of savageness and wildness in rats, J. of Animal Behav., 1913, 3, 287 ff.). Since the classical descriptions of cataplexy and of "hypnotized

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induced either under normal living conditions or at times when the rats are feeding, mating, mothering or sleeping. The demonstrated unlearnedness of the behavior would indicate that it is probably instinctive.

animals" by Preyer (Preyer, W., Die Kataplexie und der thierische Hypnotismus, 1878), and (Verworn, M., Beiträge zur Physiologie des Centralnervensystems, Erster Theil. Die sogenannte Hypnose den Thiere, 1898, Jena, pp. 1–92), much work has been done on the somewhat similar attitudes known as death-feigning. (See Severin, H. H. P., and Severin, H. C., Behavior Monog., 1911, 1, No. 3, pp. 43–44, for a bibliography of these studies). Some of our rats, however, in spite of an apparent rigidity were quite limp and inert when taken in the hand.

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THE BIOLOGICAL BASIS OF THE ASSOCIATION OF IDEAS AND THE DEVELOPMENT OF PERCEPTION

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The first part of this paper was originally published in the Johns Hopkins University Circular, March, 1914, pp. 25–41, under the title Images and Ideas. It has proved to be not sufficiently accessible, and my supply of reprints has become exhausted, so that it seems necessary to republish in order to bring it before those interested in this topic of growing interest, and to make certain additions which are now pertinent.

The two main and closely related points in this part of the paper: the mechanism of the association of ideas, and the mechanism of the development of perception; were, I believe, first presented in the article mentioned, although the scheme of the development of perception is merely a generalization of well known facts, and is substantiated by Pavloff's work on dogs, and by similar work. The suggestion in regard to the association of ideas is original; and in addition to supplying a long existing gap, it has the merit of bringing this mental type of habit-formation into line with habit formation in general, thus conforming to the scientifically imperative Law of Parsimony.

It is not unimportant to point out that the general basis on which the principles of mental habit-formation are here dealt with, while it certainly is not in accord with the so-called "structural" or the "functional" or the "genetic" psychologies, as these are usually understood, is nevertheless not "behaviorism," but may well claim the designation of *scientific* psychology. It starts with the empirical facts of consciousness as bound up with the organic activity of the animal, and attempts to unite the biological and the psychological data on the fewest and most

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general hypotheses, from which it proposes to make deductions to be tested by further observations and experiment.

The material here copied from The Johns Hopkins Circular, by permission of The Johns Hopkins Press, is reprinted *verbatim* with the omission of some of the footnotes, excepting that the term *reaction* is substituted for "reflex" and *reaction arc* for "reflex arc." This change is necessary because of the confusion which has resulted from the attempt to wrest "reflex" from its older and established meaning, and is one which I hope will be

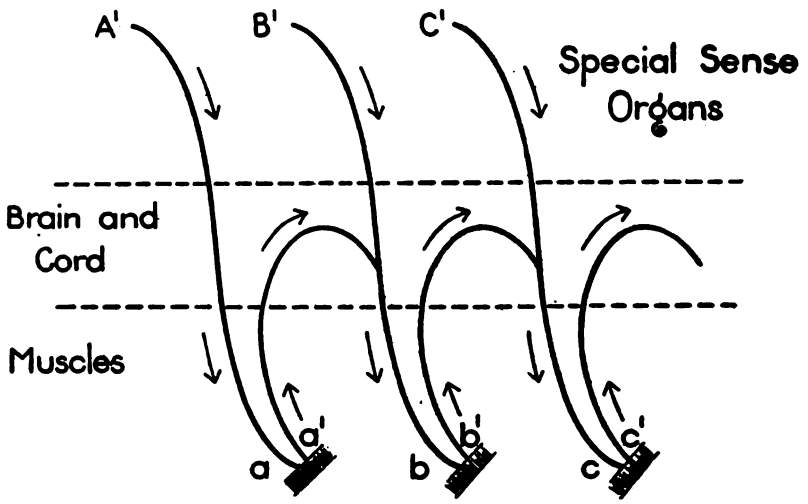

Fig. 1

generally adopted. The term "reflex" should be used to designate a certain class of reactions only, namely, those which are least variable, since the flavor of that meaning persists in clinging to the term in spite of the attempts of various authors to give it a wider definition.

More changes in the wording would be appropriate to the contemporaneous discussion of the topic, but do not seem essential. If I were writing this part of the paper anew, I should discard the term "sensation," since it does not seem possible to give it a fixed definite meaning in psychology or physiology, and in spite

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of my careful definition some readers have persisted in taking it in one of its several other meanings, and have therefore misunderstood me.

The original presentation is bound up with the discussion of the nature of the "image" which is now largely a matter of historical interest: the hypothesis may however be entirely separated from the old "image" problem without detriment.

Figure 1 has been redrawn with minor changes. A pictorial improvement might be made by representing neurons and synapses (Watson, Behavior, p. 274, has so modified my original figure), but the result would be no more true to life than the diagrams given, and may well await the accumulation of more definite information concerning the number of neurons enchained in typical reaction arcs, and the details of their arrangement in the series. Figure 2 has been redrawn in a different way, avoiding the specification of a definite time order of the original reactions.

The topic of the original paper (Images and Ideas) was continued in a paper on Thought-content and Feeling, published in the Psychological Review, 1916, xxiii, 49–70, and the latter paper should be read in the light of the present one, as it was intended to be in the light of the first one.

In the second part of this paper, in addition to more complete illustration of the hypothesis set forth in the first part, the supplementary hypothesis of "short-circuiting," which I briefly stated my Outline of Psychobiology (p. 125, 2d edition, 1917), is developed. This supplement removes the obvious objections to the original unqualified hypothesis, and brings it into close agreement with the fact so far observed.

I

Seven years ago[1] I began to tell my students that the conventional doctrine of "mental images" is, in my estimation, largely fiction, and to direct their attention to a simpler and more empirical analysis of the process and content in imagination. Two years ago[2] I published a brief text-book in which I expressed this

[1] In 1907.
[2] In 1912.

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radical view: but as the reception by other psychologists of its earlier informal expressions had not been highly encouraging, I put it in print with the least disturbance of terminology consistent with honesty; retaining the term "image," but defining it

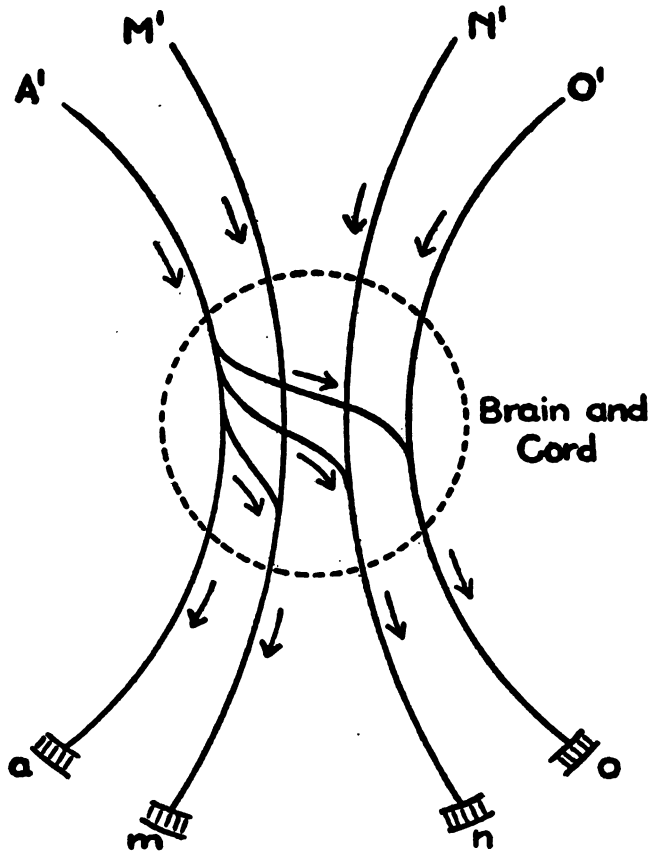

Fig. 2

to mean not a specific sort of content, but rather any content of which one is conscious in the specific way which is commonly called "thinking" as opposed to "perceiving," and pointing out that those who could not give up the official theory might con-

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tinue to use the term in the old way. This procedure enabled me to discuss perception, memory and kindred topics so that the statements were made true, whether the reader accepted my view or adhered to the older one: a result which, at that time, it was desirable to obtain.

It is now time to make a full break with the conventional theory of imagery, and to state the empirical doctrine above mentioned in the most positive and unequivocal way possible. This course is rendered advisable by the fact that some of the critical readers of my text-book were puzzled by what appeared to them vacillation in the treatment of imagination, and is rendered imperative by the recent developments in what is called "behaviorism," in which the rejection of imagery is coupled with an extreme development of empirical thought-analysis which makes my system seem now quite conservative. I am obliged to protest against the behaviorist doctrine of thought since I feel that the more conservative innovation may suffer from the opposition which will be called out by the extreme doctrine.

We must distinguish more carefully than has heretofore been customary, between "consciousness" and "content." "Consciousness" is *awareness* of anything whatever, and "content" is the anything *of which* one is conscious or aware. The distinction is perfectly clear but heretofore psychology has avoided it. "Sensation," for example, has been used convertibly for both an elementary quality of content and for the awareness of that quality. So with the other "elementary forms of consciousness" —affective and conative factors: one is rarely certain whether an author means actually consciousness, or content, when he refers to them.

This distinction between consciousness and content must be kept in mind throughout this discussion, or else much of it will be misunderstood. The term sensation, in particular, is always to be taken as indicating a perceived (or perceptible) factor, and never the perceiving thereof. When I speak of "muscular sensation" I mean the peculiar aspect of the actual muscle-contraction which is perceived by the owner of the muscle, and by him alone. The contraction has visible aspects, and tangible aspects,

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which may be perceived by several people: in addition it has this "kinaesthetic" aspect which can be perceived by one person only.

An image, as understood in current psychology, is a form of sensory content, though not exactly a sensation. Just how it is supposed to differ from a sensation is not at the present moment an important consideration, since there is a variety of opinion on the point. The fundamental likeness of image to sensation consists in the image having the modality of the sensation from which it is derived. Some images, accordingly, are visual, some are auditory, some are gustatory, and so on. This modality of images is usually understood as being the fact that the images, in themselves, differ qualitatively in the same way in which sensations differ. For example, the fundamental difference between visual images and auditory images is of the same order as the difference between visual and auditory sensation. An "idea" is commonly defined as "an image with its meaning." I may, it is supposed, have an "image" of a dark brown rectangle with a gold design on it; but that image has no more value in thought than the stars I see when I bump my head against the steampipe in the basement, unless I have also the consciousness that the colored rectangle stands for, or refers to, a copy of Hobbes's Leviathan lying on my table, or some such object other than the image itself.

Of what use, we might ask is the image? What is its function in the process of ideation? Since, in addition to being conscious of the image, I must also be conscious of the object to which it refers, should I not get on just as well if I were conscious of the object alone? Or rather, should I not get along better, since I should then have but one thing to deal with instead of two? From the point of view of the conventional doctrine of images, the only possible answer is that as a matter of fact the image does appear when one thinks of the object, and therefore that it must have some function. It seems fair to assume that the doctrine of images would never have been developed in its elaborate form, nor have been so tenaciously held, if there were not actually some present content inseparably associated with the thought

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of an absent object. What may be this factor at the basis of the conventional image theory, it is our business to find out. It is also probable that the doctrine of images is acceptable because it aids us to minimize the consideration of a remarkable and puzzling peculiarity of consciousness: the fact, namely, that we can be conscious of what is not present either in space or in time. I can be conscious of objects which are so far removed that they can exert no appreciable influence on my body, or which even no longer exist. Not only, I repeat, may the past influences of such an object on my body in certain cases influence my present behavior,—a fact which may be given a purely physiological explanation; but the object can be in or before my consciousness; that is to say, I think of it,—a fact of which psychology alone can take scientific cognizance.

Psychology, however, has shrunk from the acknowledgment of this transcendant power of consciousness and has turned her attention almost exclusively to the forms of experience which are seemingly more commonplace, namely, the experience of the present or immediate contents of the thought. It has, therefore, made much of the image and has ascribed to it certain characteristics which belong really not to the image, but to the ultimate object of thought.

I contend that the image, as a copy or reproduction of sensation of variable mode does not exist. There is indeed a present content essentially connected with imagination or thought; but this present content is in each case a muscle sensation, or a complex of muscle sensations. We are, therefore, in investigating images, dealing not with copies, or pale ghosts of former sensations but with actual present sensations.

The image, defined as a mere shadow of an auditory object, a visual object, or an object of some other mode of sense, has no discoverable explanatory function, even if the existence of such an image be admitted. But the muscle sensation renders an explanatory service which is badly needed in psychology. In order to demonstrate this, let us turn to the essential condition of consciousness; the arc-reaction.

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The unit of psycho-physiological activity is a reaction over an *arc*, which starts from a sensory neuron terminal, or *receptor*,[3] passes across two or more synapses, and terminates in a modification of the activity of one of the *effectors*, of which there are three classes, (a) striped muscle, (b) smooth and cardiac muscle, and (d) glands. No consciousness occurs without a complete reaction, although certain reactions apparently produce no consciousness. The difference between the psycho-physiological and the purely physiological reactions[4] is an important subject for investigation, but not urgent for the present discussion, in which we are assuming nothing concerning the reactions which can conceivably be altered be any findings with regard to the difference mentioned.

In actual life there are no simple arcs. Currents sent in over different afferent routes are collected in the centers and redistributed over many efferent routes. It is nevertheless legitimate to describe the neuro-muscular functions analytically in terms of simple or unitary arcs and reactions.

Having regard to the termini of the arcs, we can distinguish three kinds of reactions: striped-muscular, smooth-muscular, and glandular. Having regard to both starting places and termini, we shall find it important to distinguish between the arcs which connect similar structures, and which accordingly may be called *homeodetic*, and those which connect dissimilar structures, and which may accordingly be called *heterodetic*. We have at present no reason to assume that smooth-muscular and glandular reactions may not be psychological: but we are certain that the striped-muscular reactions have a large share in conditioning

[3] The "receptors" are: the rod-cells and cone-cells of the retina; the hair-cells of the internal ear; the gustatory cells of the taste-buds; the olfactory cells; the various corpuscles and bulbs in which sensory nerve fibers terminate in the skin, mucous membrane, and connective tissue: the "free" endings of sensory fibers in various tissues; and the *muscle-spindles* which lie in the voluntary (striped) muscles and are the specific receptors for the "muscle sense." There are apparently no sensory endings in glands, and it is questionable whether the afferent terminals in connection with smooth muscle are *normally* sensory; i.e., whether normally than can initiate *conscious* reflexes.

[4] By "purely physiological" reactions I mean those reactions which do not directly produce, or condition, consciousness.

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consciousness, and, as I shall attempt to show, that they are the essential mechanisms for associative thinking; hence we may for the time being neglect the first mentioned classes, concerning which, as a matter of fact, we possess little information.[5]

Heterodetic arcs may terminate in any of the three classes of effectors; homeodetic arcs are perhaps of the muscular types only, since there have been no afferent terminals discovered in glands. The peculiarity of the homeodetic arc is that the effect of one reaction initiates another; a heterodetic muscular reaction may therefore be followed by a sequence of homeodetic reactions—a sequence which will be brought to an end by another heterodetic reaction, from muscle to gland; or by "drainage" into another arc system; or possibly by a positive inhibition reaction.

Restricting our attention once more to the cognitive or striped muscle arcs, it becomes at once apparent that the heterodetic striped-muscle arc conditions perception and the homeodetic arc conditions thought. This deduction from the hypothesis of reaction arcs provides at once two things of which psychology has long been in want; a physiological explanation of the association of ideas, and an explanation of the nature of so-called "mental images." The way in which a given series of homeodetic reactions, once established, may become a habit, thus conditioning an associative train of thought, is so obvious that we need

[5] The smooth muscles and the glands undoubtedly have important psychophysiological function, but the details can not at present be determined. It is possible that the experiences of desire and aversion are conditioned by reactions from certain of the smooth muscles; hunger from the muscular coat of the stomach, sexual desire from the involuntary muscles of the genital organs, all other forms of desire being possibly reducible to these basic appetites and thirst—at whose reaction conditions we can guess with less plausibility. Other affective experiences may also be conditioned by reactions from smooth muscular systems, and from cardiac muscle. The muscular coats of the blood vessels, for example, have long been suspected of participation in the production of pleasure and pain. The *arrectores pilorum* in the skin may have a specific affective function. We are not at present able to declare that the *terminus ad quem* of the arc is without importance, and we must therefore admit the *possibility* that the reactions to smooth muscle and to glands may have effects on consciousness which are characteristic regardless of the *terminus a quo*. The morphology of glandular and smooth muscular arcs must be more fully known before speculation concerning the corresponding reactions can be useful.

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spend no time at present in amplifying this detail. The manner in which such a series becomes established requires some further explanation. Let A'–a, B'–b, and C'–c[6] represent heterodetic muscular arcs (that is perceptual arcs) which have become habitual. If on certain occasions we have reactions through these in succession, we may have the afferent current from a' collected by central neurons and combined with the current from B' into the discharge to b; similarly, the current from b' collected and combined with the discharge to c, and so on. As a result of this process, especially if the serial stimulation of A', B', and C' is repeated a number of times, we have the homeodetic arcs a'–b, b'–c, and so on, established as paths of habitual reactions. In other words: the sets of muscular contractions habitually associated with A', B', C' and so on, have become associated into a series.

Going back now one step farther; the reaction, although represented above as occurring in a simple arc from A' to a, is really a complicated reaction built up in a manner similar to that in which the homeodetic reflex a'–b is established. That which was originally a simple discharge from A'[7] has, in the course of many repetitions, been combined with discharges in many other arcs M'–m, N'–n, O'–o, etc., and thus not only may homeodetic arcs between a and m', n', o', etc., have been established, but also central connections between A'–a, M'–m, N'–n, O'–o, etc., have been formed. As a consequence of this central fusion a condition has been established such that a current from A' alone will be distributed, not to a alone, but also to m, n, o, etc. That is to say, stimulation of A' alone will produce somewhat, though not exactly, the same results which would earlier have been produced by the stimulation of A', M', N', O', etc.

At this point our analysis compels us to plunge into the epistomological whirlpool, but by holding fast to our reaction hypothesis we can come safely through. The heterodetic reaction A'–a, in its simplest or original form, conditions the consciousness of A, the sense quality corresponding to the stimulation A'. In the

[6] See figure 1.
[7] See figure 2.

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case of retinal stimulation, A' is the process in the cone-cell, and A is the color actually "seen." A is properly called the sensation in one of the several meanings of that term; namely, that which we are conscious of or perceive through the mediation of a sense organ. M, N, O, etc., therefore, are other sense qualities, or sensations, the consciousness of which is conditioned by the arcs M'–m, N'–n, O'–o, etc. The final, more complex, reaction from A' to a and also to m, n, o, etc., conditions therefore the perception of A, together with its associates, M, N, O, etc. In concrete illustrative terms: the visual presentation of an apple no longer arouses visual perception merely, but arouses also the perception of the gustatory, olfactory, tactual, and possibly the thermal qualities of the apple.

It might be urged that in considering the reaction as the condition of consciousness we have put the emphasis in an almost exclusive way on the efferent side; that it seems to make no difference where the reaction originates, so long as its terminations are of a certain sort. This is not an accurate deduction. The consciousness conditioned by the reaction, from A' to a, m, n, o, etc., is not the same as that conditioned by the simultaneous reactions A'–a, M'–m, N'–n, O'–o, etc. In concrete terms: the perception of the apple presented to the eye alone is not the same as the perception of the apple which is sensibly seen, tasted, smelled and felt at once or in quick succession. Perception, in other words, is not mere multiple sensory intuition, and we shall show later that it is not, on the other hand, mere sensory intuition *plus* imagination, even in an analytical interpretation.

Passing on to the topic of present chief interest: what is the content of thought? Again we have a deduction from our fundamental hypothesis which is amply verified by observation. There are two contents for every state of thought-consciousness. First, the muscular contraction which is responsible for the initiation of the homeodetic reaction—not the muscular contraction considered as a concrete whole, but that aspect of it which we call the *muscle sensation*. The contraction as a whole is visible and tangible, as well as sensible through the mediation of the muscle spindle. When we speak of contraction we intend the total of

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the properties of the process, or at least several of them, just as when we speak of an apple we mean the total, or several, properties of the object. The perception of the contraction as a complex fact may be conditioned by a reaction from the muscle over an arc built up in the way described for the complex visual arc above. The direct or immediate content of the homeodetic reaction consciousness is, however, the muscle sensation (or sensation complex). This sensation is the true *image*. Secondly, the consciousness conditioned by the homeodetic reaction may have for its ultimate or derivative object the object of the perceptual consciousness conditioned by the heterodetic arc which originally terminated in the muscular contractions initiating the homeodetic arc in question. Another way of putting it is to say that each perceptual reaction ends in a muscular contraction which causes a thought-reaction, and that the object of the perception and the indirect object of the thought are the same.

The sidelights which further deductions throw on the whole process of consciousness are of high luminosities, and I believe that what they show is fully justified by observation. Upon only one of these points need I touch in the present paper: and I discuss it only to overcome what may seem an important objection to the hypothesis as developed up to this point. The thought-consciousness is not uniformly of both the direct object and the indirect object, but is variable; sometimes it is predominantly of the direct content, sometimes of the indirect content. This variability of *attention* as we usually designate it, is without doubt due to variations in the relations of the particular arc or group of arcs in question to the totality of other arcs occurring at the same time, or in more accurate language, to the other parts of the total arc-system. This conception of the dependence of what Münsterberg has called *vividness*, and Titchener has later called *sensory clearness*, on the interrelations of arcs is not new. It has been made familiar through the work of W. McDougall in particular. This factor of interrelations also determines the formation of particular arcs, and determines which of several branches of an arc a particular reaction shall follow.

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Returning now to our chief topic, we find that attention to the direct content of thought reveals muscle sensations and only muscle sensations. The derivative content is the *idea*, and is in itself not different from a content of perceptual consciousness. The idea is of course a variable thing, just as the object of perception is variable; now this aspect being before consciousness, not that aspect. The chief difference between ideal and perceptual content is that the idea is much more variable than the percept; and this is because the system of muscular contractions is exceedingly complex, and ever recurs in constantly varying forms. An illustration of the extreme complexity of the image-contractions is found in language: a word as perceived kinaesthetically is the sole direct content which occurs in many cases of thought, and for the production of a single word a great number of muscular activities are required.

It is of course exceedingly difficult to separate completely in introspection the direct content from the idea. The difficulty is especially great if we do not understand what the direct content really is. Hence we need not be puzzled by the fact that the direct content has been described in conventional psychology as possessing the modality, and possibly other characteristics, of the idea. For example, in many cases the so-called image is classed as visual merely because the idea it controls is an idea which is primarily visual, or of which the visual features have been chiefly attended to. Under the influence of this tendency alone, persons would be distributed in types strictly in accordance with their habits of attention. But there is probably another factor which enters to make the determining of types difficult and variable. The images are operations of a great variety of bodily muscles. The muscles of the face, eye-balls and vocal organs participate in imagery to a very important degree; the muscles of the arms and upper part of the trunk have less to do, and the image-functions of the legs are perhaps still less important. The sexual organs sustain a certain amount of imaginative activity which, when it occurs, is very definite. The muscles of the organs of the special senses are in many cases concerned in "imagery," and there is a strong tendency in

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these cases to refer the image to the mode of the special sense concerned. If the muscles of the eye are involved in the production of an image, there is a tendency to classify the "image" as visual, and so on. Unquestionably, the use of the vocal organs becomes more pronounced as we advance in age and education, and the "images" which would be naïvely classed as visual and auditory become less consequential. Olfactory "images" ought at all times to be infrequent, since the muscles involved in the act of smelling are not specialized to that function.

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II

The hypothesis presented in the preceding discussion stands upon a postulate which fertile psychological investigation finds increasingly difficult to avoid. This postulate, briefly stated is: *habits depend on laws of operation of the nervous system which are the same for habits of perception, habits of thought, and habits of action, because both perception and thought are primarily details in reactions.* In this brief formulation importance must be given to the word "primarily," since, as will be explained later, there may be certain features of perception and thought in the final stages of habits of perceiving and thinking, in which they differ from the same habits in the earlier stages. Throughout the field of habit formation we may expect to find that considerable changes in the mechanism of complicated activities take place in the stages between the primary and the final operations of learning.

It is postulated, moreover, that consciousness, whether perceptual or ideational, depends upon[8] *reaction*, which is primarily a discharge from *receptors*, through the central nervous system to *effectors*, and terminating in specific activity of those effectors. The route over which the discharge travels is designated a *reaction-arc* (the new term being preferable to the older "reflex-arc," which is retained to designate a limited type of reaction-arc). There will be noticed, in the discussion, a certain amount of con-

[8] One may say instead of "depends on," is conditioned by, is caused by, or, is a part of a total process involving: according to one's private brand of philosophy.

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fusion due to the lack of a specific term to indicate the merely neural part of the reaction: the discharge over the arc, exclusive of the effector activity in which it terminates. It seems proper therefore at this point to introduce the term *transit* (or *neural transit*) to designate that part of the reaction which includes the discharges of the successive neurons in the chain including the receptors and leading up to, but not including, the ultimate effector activity. *Reaction* will be used as usual, to include both the transit and the terminal activity, muscular or glandular.

It is assumed moreover, that consciousness is dependent, not on reactions or neural transits merely, but on the integration of the nervous system; the higher (more vivid or more attentive) degrees of consciousness depending on the more complete integrations. This assumption is not indispensable for the exposition of the main hypothesis, but makes much clearer the bearings of the further suggestions made below.

Two illustrations of the serial connection of reactions may be offered: learning to waltz; and learning, or "memorizing" a list of words. In the first case, an action-habit is being formed: in the second, a thought-habit is ultimately established. In learning to waltz, a series of reactions are to be connected so that they follow mechanically in the proper sequence: in learning a series of words, the process long known as the "association of ideas" is exemplified. Neither process is really simple, as it starts from a complex of habits already formed, and the stimulations which operate in the formation of the habit are also complicated: but we may legitimately conclude that the habits already in existence were formed in the same way as that in which those under consideration. An attempt to start from conditions really simpler, that is, conditions in infancy, is fallacious because we can usefully interpret the learning of the infant only on the basis of the examination of better known conditions.

The first reaction in waltzing,[9] for the man, concludes in drawing the left foot straight back: the second, in drawing the right

[9] The details given are for the old or "standard" waltz, not the at present more popular "skip" waltz, in which, as in the "two-step" (which of course is really a three-step) the feet are brought together by the second step, and separated on the third.

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foot diagonally back and to the right: the third, in moving the left foot laterally over to the right: the fourth, in advancing the right foot straight forward: the fifth, in advancing the left foot diagonally forward and to the left; and the sixth, in moving the right foot laterally over to the left. At this point the series commences again and may be kept up without change for a certain length of time, when, by moving the right foot backwards instead of forward on a fourth step, the series is *reversed*, this step forming the first in a new series in which the right foot goes back at *one* and the left foot forward at *four*. This series must be modified by turning as the steps are taken, so that the absolute directions of "forward" and "back" are constantly changing, and the relative directions and lengths of steps must be modified in accordance with the needs of the floor and the activities of other dancers: but the first thing which has to be done, if waltzing is to be learned quickly and effectively, is to form the two series (direct and reverse) of six steps each, and make the series mechanically perfect. We may therefore concentrate our exposition on these series.

In learning the waltz steps, each step is first initiated separately, as the result of an elaborate system of thought and perceptual reflexes. A trained waltzer however initiates only the first step, and if the floor is ample and progress unimpeded, he may give his attention to conversation with his partner for some time, the series of steps taking care of itself. The stages in the progression of the habit up to this point of perfection are material for analysis.

In figure 1, the lines A'–a, B'–b, C'–c, represent the neural arcs involved in the first, second and third steps respectively of the waltz series. The arcs are of course really multiple, each complete transit being made up of transits from many receptors, over many parallel neuron chains, to many effectors (fibers of leg and trunk muscles); but the multiple transit and arc in each step may be legitimately represented by a single arc, since the principles involved would apply as well to cases (if such were possible) in which a single receptor discharges to a single effector over a single-track arc. Analytic simplification of this sort is necessary as well as customary.

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The stimuli in the several cases, we may suppose to be the words "right," "left," and "over," addressed to the pupil's auditory receptors; or may be significant movements of the instructor's hands affecting the visual receptors; or tactual stimuli applied by the instructor's hands; or combinations of these. As learning progresses, ideational rather than perceptual processes become increasingly important, but these, too, depend on stimuli of the receptors of some sense, or senses, hence we may designate the total stimulations in the three reactions as A, B, and C, and designate the corresponding processes in the receptors of the senses involved as A', B', and C'.

The muscular contractions (and relaxations) in the three reactions may be represented, as if there were only one muscle-cell involved in each, by a, b, and c.

Changes in the muscles in contraction a, stimulate the receptors in the muscle spindles with which all striped muscle is provided. Afferent current from a', the process in the muscle-receptors, is thus sent into the "centers" (spinal cord and brain), reissuing, in accordance with habits already established, over some efferent route (not represented in Fig. 1) and thus forming a new neural circuit, the transit over which conditions the consciousness of the leg movement. In the early stages of learning this transit may dominate the integration of the total nervous system for the moment, giving rise to vivid (attentive) consciousness of the movement. By degrees this dominating tendency of the transit disappears, until finally this particular transit is so completely absorbed in the integrations dominated by other transits that there is practically no consciousness of the leg movement as a distinct entity.

Since the transit beginning with A' has a certain duration, and its individual function is brief, its residual activity is "drained" into the circuit of the next important transit, B'–b. By the concept of "drainage" above assumed, we mean at this time nothing more than that a functional connection is established between the arc commencing with a' and the new arc B'–b. It may be true that in the establishment of this connection "current" already flowing over the a' arc is actually diverted to the B'–b circuit

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(whence the term "drainage"), so that a transit a'–b is actually established at the moment, but this need not be assumed. The important fact is that a condition of functional connection is established so that at the next occurrence of a centripetal discharge from a' there will be a tendency for an arc from a' to b to be actualized: and if the process be repeated several times the arc will certainly be established, and the stimulation of a' will produce the movement b through the direct transit a'–b, with no need of the sensory processes B' at all.

The linking together of b and c, representing the second and third steps in the waltz; and the linking of the third and fourth, fourth and fifth, and fifth and sixth; follow the same course as the linking of a and b. Eventually, the series is so completely associated that given the proper start, it will continue with no necessary further stimulations than those supplied by the muscular contractions themselves. And this result follows from the general properties of neurons as assumed under the concepts of integration and drainage, needing no special "association centers," or of any groups of nerve cells possessing functional properties differing in any way from the properties of all nerve cells (excepting in so far as receptors may constitute such a class).

Obviously, the first connections of the afferent and the efferent parts of the circuits from muscle to muscle will be established in that part of the central nervous system in which the synaptic contacts are most numerous and complicated, namely, the cerebrum: the great central automatic switch board.

It is not to be supposed that during the fixing and perfecting of the habit by continued practice changes in the route of the circuit do not occur. Possibly the circuit as first established is later very much shortened; possibly some circuits are so shortened and modified that when the habit is perfected the route no longer passes through the cerebrum. On these points there is little information available.

The "proper start" above specified as the prerequisite for the operation of the completed habit must not be simply conceived. The mere completion of the first step in the waltz series is not a sufficient "start" for that series: if it were, an expert waltzer

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would begin to waltz whenever he happened to take a step backward with the left foot. Obviously, other stimulations are needed to put the total nervous system into the condition of integration in which the waltz series will be repeated, rather than one of many other series. These stimulations are normally furnished by the music,[10] the contact with a partner, and other environmental features of the ball-room. Thought-processes, including intentions, which are also essentially reactions depending on neural transits are important in selecting the habit to be actualized; and in the absence of any of the customary environmental influences the thought-processes alone may start the waltz series: otherwise, the pupil would be unable to practice the steps by himself.

It is not to be supposed that the problems of habit formation are solved by the muscle series hypothesis, or that this hypothesis is put forward as more than a step in the total solution. Yet it is an important step, and once taken, the way is cleared for concentration on the problems of selection and control of reaction series.

Let us consider now the details of the formation of a thought-habit, basing our analysis on the memorizing of the following list of words: *coffee, quantity, brittle, aggravate, paper, sunny*. Suppose these words to be presented for learning, either by sounds addressed to the auditory receptors, or by printed letter-combinations addressed to the visual receptors. The perception of each of these words depends on a complicated reaction which we will suppose to have already been learned by the person who is to

[10] Unfortunately, to an increasingly large number of would-be dancers, the music is no clue. The word "waltz" either displayed on a program or sign, or obtained verbally from some other person, is the necessary stimulus; and the dancing proceeds in a personal time-series, having no relation to the rhythm of the music. In a modern ball room, the observer will notice that the number of steps per minute of a large percentage of the dancers is not determined by the rate of the musical rhythm; and may confirm his visual observation by the auditory rhythm of the feet, and the lack of vibration of the floor which always marks unison dancing. This sort of "dancing" is a much simpler illustration of the serial habit formation explained above than is real dancing, in which afferent auditory current is continually participating in the series, no matter how completely it may have been learned.

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memorize the series: that is, the words are already familiar, as words; but the series has not been learned.

The exact reaction for each of these words will vary according to the person perceiving, and according to his age and stage of education. It might be assumed that the reaction will in every case terminate in vocal activity, but this assumption is very probably incorrect. In very many cases however, possibly in the majority of cases, the reaction will be vocal, and we may use this reaction to illustrate the process of association, which will be the same in principle whether the reactions are vocal, or whether they terminate in movements of the fingers and hand (as in writing), or of muscles of other part of the body. The reactions must in every case be muscular in termination, or no associations can be formed between them.

The simple diagram of figure 1 will serve to illustrate the serial connection of the words as well as it illustrated the connection of the dance movements. Let A, B, and C be the first three stimulus words: then A', B', and C' are the receptor-processes initiated by these stimulations, and the lines A'–a, B'–b, and C'–c are the arcs of the neural transits which terminate in the speaking (audibly or silently) of the words. Since the several muscles of the throat, face, and neck which participate in the pronunciation of words are supplied with sensory nerve terminals, the linking together of these reactions occurs precisely as does the linking together of the steps of the waltz. The afferent current initiated at a' by the contraction a of the first word, is drained off in the cerebrum into the circuit B'–b of the second word: the afferent current from b' is drained into the circuit C'–c of the third word, and so on. Ultimately, the series is so linked together that, given the proper start, the series of words will be repeated accurately with no further necessary stimulation either by the sound or sight of the words.

The association of words may be facilitated by factors which as in the case of the waltz, control the selection of terms. The series *coffee, tea, sugar, cream, spoon, cup,* will be facilitated by previously formed associations between the terms. The learning of the series *bean, lean, queen, seen, mean, green,* will be affected

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(not necessarily simplified) by the similarity of the reactions, and possibly by the similarity of the stimulations. In learning significant series, of prose or poetry, still greater complications occur, as the words are not associated as simple terms but as significant groups: that is, complex serial habits already formed are woven into a new series with attendant inhibitions and facilitations. If the several reactions themselves have to be learned as well as the serial connection, as in memorizing series of "nonsense syllables" such as *nef, yok, pib, ruz, mog, zal,* the learning is more complicated than in learning a series of words already familiar.[11]

The probable changes in the mechanism of serial connections during the learning period, to which we have already referred, must now be considered more concretely. Complete reactions are apparently essential during the learning process; during the learning of serial connections as well as the learning of perceptions. Action, perception, and thought, in so far as they are learned at all, are learned through action. There is however no evidence that in the final stages of thought-habits and perception-habits, action is essential: nor is it probable that in serially connected activities, as in knitting or waltzing, the completion of one action in the chain is the actual stimulus for the next, after the series has been learned to a reasonable degree of efficiency. It may possibly be, of course, that complete reactions always occur, and that each member of the series waits on the completion of the preceding one: but the facts do not at present appear to be such, and such a mechanism, however admirable for purposes of learning, seems needlessly clumsy and inefficient for the finished result.

It is probable that during the learning process for serial activities and the association of ideas, *short circuits* are progressively developed, and simplify the reaction chains very much. It is not safe to extend the reaction hypothesis, as applied to learning processes, beyond the learning period; and it is necessary to search

[11] This is an important point which has to an unfortunate extent been overlooked. We can not expect the learning curve for nonsense syllables to follow in all respects the laws of the simpler process of learning significant words, any more than we can expect serial association and paired association to give exactly similar curves—although there is even a flagrant example of the latter expectation in the German literature.

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for the mechanism which is capable of providing the short circuits.

At the present time, the cerebellum is the organ to which the supplementation of the muscular system may most plausibly be attributed, and its known connection with the muscular activities and with learning render it advisable to consider the probable cerebellar functions before seeking elsewhere for a short circuiting mechanism. The accumulated knowledge concerning the cerebellum is too voluminous, and too full of difficulties to be discussed here in any detail, and I shall attempt only the sketching of the scheme which at present seems most significant.[12]

Whatever the principal functions of the cerebellum in the lower animals, it may be assumed in man to have acquired the possibility of receiving efferent current from the cerebrum, and of redirecting current back to the cerebrum, just as the striped muscles are known to do. Assuming further the development of a "point to point" correspondence between the cerebellum and the striated muscular system,[13] we would have the possibility of a split in the efferent leg of the primary reaction-arc, so that current might be sent *both* to the muscle or group of muscles and to the corresponding cerebellar cells. Afferent current (to the cerebrum) from these cerebellar cells would tend to be drained into the channels of the succeeding reaction transits in accordance with the fundamental principles of integration. A series of neural transits would therefore be established paralleling the series of complete reactions represented in figure 1; a series of transits between the cerebellum and the cerebrum: and the establishment of that system would have very important consequences.

[12] For summaries of observations and theories, Luciani, Physiology, vol. III, chap. VIII, and André-Thomas, Cerebellar Functions translated by W. Conyers Herring, (Nervous and Mental Disease Monograph No. 12), should be consulted.

[13] This "point to point" correspondence is not necessarily anatomical. It may be a correspondence between certain cerebellar cell-groups and certain definite muscular functions: or it may be entirely functional. The relations between cerebellar cells and other portions of the body is known to be of a sort which is easily modified, one part of the cerebellum being capable of assuming functions previously exercised by another.

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In the first place: in a series of activities of the muscles, the successive reactions would not have to wait for their initiation, on the completion of the preceding reactions, but could be initiated from the shorter cerebellar transit. A great saving, especially in a rapid series, would thus be accomplished, although the afferent current from the completed reactions would always serve as a check on the series, in case of its going wrong or suddenly needing to be modified.

In the second place: in a series of associated ideas, the muscular activities would not be needed at all, after the series had been efficiently formed; the series of discharges between the cerebellum and the cerebrum being entirely sufficient, since they integrate, and are integrated with, the total neural activity as thoroughly as are the primitive reactions involving the muscles. For consciousness therefore, the cerebro-cerebellar system suffices (after it has once been established by reactions), except in so far as further modifications are necessary in the series, or as a slight renewal of the learning process may be necessary to offset *forgetting:* little further modification of the series being possible after it has been handed over to the shorter system.[14] We should expect to find, however, vestiges of the original muscular activi-

[14] Excepting of course deterioriations. As is well known, all series tend to fall to pieces in time, although certain series resist the tendency to a surprising extent. The extent to which modifications in old series, or the formation of new series, may occur without muscular intervention—if such modification or formation be possible at all—is an important field for experimental work. The phenomena of temporary memory, as in the transcribing of words or figures offers a good starting point. In listing nine-place numbers on an adding machine, where the numbers are read from a sheet at the left of the machine, errors are excluded if the numbers are distinctly articulated as read: without distinct articulation it is apparently impossible for the ordinary operator to avoid errors unless he refers back to the sheet once or more during the listing of a number. The question here is whether actual articulation is not essential in all such cases, even for two place numbers; or whether a certain (and manifestly inferior) association is possible without articulation. In such cases, the possibility of finger innervations functioning instead of articulation; and in the case of expert operators, functioning better; must not be overlooked. Serious fallacies result from the consideration of vocal movements as the sole motor components in thought-processes: the whole striated musculature has important thought-functions, although the vocal muscles have, in intelligent adults, the leading rôle.

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ties present in many cases, although they are no longer useful, and although they are too slight and fragmentary to serve as actual stimuli for the succeeding transits. Such vestiges are in fact frequently noticed, as in the lip and throat movements which in many persons accompany silent reading, and have been demonstrated by laboratory technique in cases where they were not apparent to casual observation.

We should expect, if this scheme represents the actual facts, to find that abolition of the cerebellar functions abolishes serial habits which have previously been established: and this is apparently what actually happens. We should expect further, to find that after abolition of cerebellar functions, learning is still possible, but that it will never reach a normal level of efficiency: and this also seems to be borne out by observations already made, for action at least. Since all habits follow the same general laws we may confidently expect to find the same results in the case of thought-habits.

The consideration of the cerebellum as an organ of thought, or rather as an important part of the thought mechanism, is perhaps a startling novelty; but it contradicts none of the known facts of cerebellar function, and necessitates little essential readjustment of present views concerning it. The relatively larger development of the cerebellum in lower animals as compared with man is a point which, on first consideration, seems to stand in opposition to the hypothesis; but in reality does not. Not only may the cerebellum be supposed to have functions not strictly included in the associative process—such for example, as the regulation of muscular tonicity, in conjunction with the semicircular-canal receptors: but also, the proportion of completely habitual (mechanized) activity to variable activity must be assumed to be much larger in the animal than in man. If animals have thought (and there is no reason to suppose they have not), it very probably is more fixed in its sequences than is human thought.

Whether the cerebellum is the organ responsible for the short-circuiting of serially connected reactions, or whether the mechanism must be sought elsewhere, it is plain that the reaction-hy-

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pothesis as applied to the association of ideas, and to thought in general, must not be taken in bare simplicity. For the present, it should be restricted to the learning process, for the explanation of which I introduced it. Learning is obviously a process the ideal of which is its own abolition, and it would be surprising if we did not find the final mechanism of association characteristically different from that essential to the initiation of the association process.

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STUDIES OF CEREBRAL FUNCTION IN LEARNING

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I. THE PROBLEM OF NERVOUS FUNCTION IN LEARNING

General questions

We are, most of us, accustomed to think of learning in terms of nerve cells, synapses, changes in conduction of nerve impulses, etc., but it must be borne in mind that some recent writers deny

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the necessity for any specialized structures and, indeed, consider all learning, even of the conditioned reflex type, to be an inherent function of protoplasm. This view has been advanced chiefly by Neo-Lamarckians beginning with Hering and Butler and extending to Semon, Driesch, Rignano, and F. Darwin among recent writers. The great mass of evidence upon which their contentions are based is without firm foundation, consisting as it does of inferences drawn from evolutionary series, and the few experimental facts which are adduced, such as the results of Schroeder and Kammerer, lack verification. Even if established they would not bear directly upon the relation of somatic induction to learning. Nevertheless, the contention for the possibility of extra-neural learning can not be dismissed summarily upon the basis of established facts. The most recent contribution toward a theory of extra-neural learning is that of Kappers ('17) in which it is assumed that excitatory processes are conducted through non-nervous tissue, directing growth processes and nervous integration. The doctrine (neurobiotaxis) seems to have been received favorably by many neurologists. It is, however, based wholly upon deductions from anatomical data. Nowhere in the studies of growth or evolution has a case of learning been actually observed.

Have we any evidence that learning in the individual is possible in the absence of differentiated conduction paths? A few years ago one might have been tempted to say that a demonstration of learning in protista would give a positive answer to this question. But the recent publications of Sharp ('14), Yocom ('18), Taylor ('19) and others suggest that protista have well differentiated conduction systems. Even in Amoeba, in which there is no indication of structural differentiations of this character, the observations of Kepner and Taliaferro ('13) show that reactions may occur at a point far distant from the point of stimulation, as when stimulation between two pseudopods results in change in direction of movement at their tips. So, while some observations, particularly those of Metalnikow ('12), indicate that the protozoa are capable of forming habits, we must conclude that there is no known case of learning in

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an organism which is not equipped with a differentiated conduction system.

The theories of the mnemonic nature of growth and evolution and of extra-neural learning in general need therefore not detain us further here. There is certainly need for continued investigations in these fields, but whatever the condition in embryonic tissues may be, it is evident that learning in the individual is chronologically and functionally first and that in all metazoa, at least, learning is coexistent with differentiated conductive tissue.

Whether or not learning is restricted solely to the activity of this tissue is, however, not certainly established. It has been suggested by the writer ('17) and by Dunlap ('17) that endocrine activity may play a part in the fixation of habit but some recent experiments (to be reported in another paper) make this seem improbable. There is no conclusive evidence for the general activity of any extra-neural structure in learning while there is clear evidence that nervous organization is of fundamental importance for the process. Upon the mechanism of nervous activity in learning we have no direct observations but studies of the mechanism of nervous conduction in general define rather clearly the type of activity which we may expect to find.

The most important neurological concept bearing upon nervous function in learning is that of the reflex character of all behavior. Theories of the fixation of "traces" in particular cells or areas of the brain and of the arousal of images by the excitation of these cells or areas have now little more than historical interest. The doctrine of the image seems pretty well exploded and the vast amount of data upon the reflex nature of spinal and bulbar reactions accumulated within recent years leaves little room for doubt that every reaction of the organism is carried out by transmission of impulses over reflex paths differing only in the number of cells and the complexity of organization intervening between receptor and effector. The problem of nervous function in memory is, then, no longer one of the locus in which memories are stored; we can not hypothetize with Ferrier ('76)

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ideas of movement stored in the motor area and ideas of sensation in the sensory areas, or with Loeb ('00) resonant vibrations involving the whole of the brain. The trained organism differs from the untrained in its reflex reactions and it is the task of physiology to trace through the reflex paths which determine these reactions and to describe the mechanism by which the direction of conduction is altered.

The problem here seems clear enough but methods of solving it are far to seek. We can not trace nerve impulses directly; we can not make chemical analysis of individual nerve cells and even if this were possible we could not interpret the results until we had some notion of what cells are functional in learning. An attempt at correlation of the learning function with structural differentiations of the nervous system seems to offer almost the only present means of attacking the problem. Is the capacity for formation of new functional connections inherent in all nervous tissue, or is it characteristic only of a certain type of organization? Does reintegration occur in the simple nerve net or only after the appearance of synaptic connections? In what portions of the reflex arc and in what gross divisions of the nervous system does reintegration occur? Through what portions of the nervous system are the propagated impulses of learned reactions transmitted? Is there any type of nervous organization which is particularly favorable to learning? Answers to these and similar questions may help us to formulate experimental problems bearing more directly upon the mechanism of fixation of habits.

The questions which yield most easily to experimental attack are those dealing with the function of gross anatomical divisions of the nervous system. A good bit of evidence bearing upon them has already been accumulated but its inconclusive character may be judged from the conflicting opinions of recent investigators. One finds statements to the effect that learning occurs only as a function of the cerebrum (Loeb, '00; Lloyd Morgan, '12), that it may occur also in lower centers (Luciani, '15; Bechterew, '09), that it is the result of the combined action of all parts of the cerebrum (Loeb, '00), that it is a function of

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a particular part of the cerebrum, (Sollier, '00; Franz, '07). Many of these and similar statements are based upon purely metaphysical grounds, as Morgan's doctrine of the cerebrum as the seat of consciousness. Others are based upon an inadequate experimental analysis of the problem, and much more experimental data must be accumulated before we can subscribe fully to any of the theories of nervous function in learning.

Before we outline the problems of nervous localization in learning and methods for attacking them it is necessary to recognize clearly that learning and retention may result from the activity of distinct nervous mechanisms. It is conceivable that the conditioned-reflex paths which mediate the learned reaction are established in one portion of the nervous system, under the directive influence of some other part of the nervous system which functions only during the learning process and is unnecessary for the production of the learned reaction. The hypotheses which imply that learning requires the conscious direction of the cerebrum although learned reactions may be carried out altogether at subcortical levels imply such a condition. On the other hand the attempts made by Max Meyer ('11), Watson ('14), and others to account for learning in terms of reintegration of simple reflex arcs assume that no other nervous organization is necessary for learning than that included in the reflex arcs which are reintegrated and communicating cells between them. There is no certain evidence to support either one of these hypotheses and the question can be settled only by a detailed examination of the function of every part of the nervous system during learning and in the performance of learned reactions. Before this is carried out extreme care in distinguishing learning and retention in interpreting experimental data will be necessary.

The greater part of the existing experimental and theoretical work centers about the problem of the function of the cerebrum in learning. Since the present investigation deals with certain aspects of this problem a brief review of the experimental work which has been done upon cerebral function in learning may serve to connect the somewhat detached sections of the paper and bring them into relation to the fundamental problems.

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A. The question of prime importance for habit theory is that of the existence of anatomical divisions of the nervous system to which the function of learning is restricted. The discovery of such a region would give an opportunity for the study of the rôle of finer structural differentiations, but there is no real evidence that any such specialization exists. The evidence advanced in support of the contention that the cerebrum of mammals is the sole seat of the learning process is this:

1. Invertebrates, lacking a cerebrum are incapable of associative memory (Loeb, '00). This argument was advanced before many tests of lower animals had been made. The demonstration of complex learning in all phyla above the coelenterates annuls it.

2. Total destruction of the cerebrum is followed by the loss of most of the learned reactions of the organism. The observations of Flourens, Schrader, Goltz, Rothmann, and many others establish this beyond a doubt. But in no case have the observations upon decerebrate animals been sufficiently detailed to prove that all habitual acts are abolished by the operation. The average habitual acts observed by investigators have been of a complex type requiring tremendously complex coordinations of reactions to kinesthetic and external stimuli and no data are available upon the persistence of simple conditioned reflexes. The experiments of Rothmann (cited below) show either the retention of some habits after complete decerebration or their reacquirement after the operation: in either case a strong argument against the limitation of learning to the cerebrum. Lashley and Franz ('17) found that simple habits were retained by the rat after destruction of very large portions of the cerebrum, embracing all except the temporal and basal portions. Finally the experiments reported in section V of this paper show that a simple habit may be retained after the destruction of any given third of the cortex.

3. The formation of habits is not possible in the complete absence of the cerebrum. In practically every case this conclusion has been advanced without any serious attempt to train the animals, on the basis of the general impression of stupid-

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ity given by them. Without careful training experiments such observations are absolutely worthless. My own experience with an animal of the present series illustrates this. The rat (number 19, section III) had extensive destruction in the frontal and parietal regions. She had a paresis of the right legs which made her walk stumbling and uncertain, disturbance of the musculature of the face and excessive length of teeth which gave her an expression of extreme stupidity. She seemed to wander about aimlessly, rarely reacting to stimuli which influenced normal animals. Several people who saw her remarked her stupidity and predicted that she would never learn the problem and this was also my impression. Yet the animal learned the difficult double-platform box in 27 trials, which is less than one fifth of the average of normal animals, and showed in overtraining tests a constancy of performance rarely equalled by normals.

But few attempts have been made to train animals after complete destruction of the cerebrum. The observations of Goltz ('92) upon his decerebrate dog extended over a period of more than eighteen months and some efforts were made to train the animal (to back out of a narrow stall) but the author feared to continue training because of the danger of producing convulsions and death by continued stimulation. The result of the tests were negative, but Goltz admits that the training was not continued long enough to justify the conclusion that the animal could not learn. Aside from the rapid recovery of walking coördinations which was probably a concomitant of recovery from shock, he reports no improvement except that of finding food. The swallowing reflex could not be elicited at first except by placing food in the pharynx. Gradually the reflex came to be called out by more distal stimuli to the tongue, chewing movements appeared, and on the twenty-fourth day after the operation food was first taken when placed against the lips. A similar improvement was reported by Rothmann ('12) who did not give the time for recovery. Rothmann further reported that his dog learned to accommodate the stepping movements of his hind legs to follow the movements of a chair

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upon which his fore feet were supported. These alterations in behavior may have been merely a result of gradual recovery from shock but they may with equal justice be interpreted as cases of simple learning.

The one outstanding experiment upon decerebrate animals is that of Burnett ('12), who failed to obtain the formation of the maze-habit by decerebrate frogs. His experiments were well controlled and seem to demonstrate that the decerebrate frog is incapable of forming so complex a habit, but they do not rule out the possibility of learning of a simpler type. As Franz and the writer have pointed out, the disorganization which follows decerebration is so great that many of the factors which are ordinarily effective in inducing learning (for instance, the sexual impulses) are no longer present and unless the training methods are adjusted very carefully to the condition of the animal negative results of training can have little significance.

The experimental work does seem to show that destruction of the cerebrum abolishes the vast majority of learned reactions and that decerebrate animals have a greatly reduced capacity for learning, but it does not show conclusively that it completely eliminates all ability to learn. This insistance upon the possibility of learning in the absence of the cerebrum may seem needless quibbling when there is so little evidence that non-cerebral learning plays any important part in the behavior of higher animals, but the point is one of the greatest importance for learning theory since upon it hinges the question of whether learning is the product of some particular kind of nervous organization or is a capacity of all integrated nervous tissue; whether the cerebrum is a highly specialized organ or is merely a mass of reflex paths comparable in all save complexity to the spinal cord.

The evidence from comparative anatomy suggests that the cerebrum is not essential for reintegration of reflex arcs and data upon the vicarious function of various parts of the cerebrum, upon retention of habits after destruction of any given part of the cerebrum, and upon learning in partially decerebrate animals all tend to substantiate this view. A final judgment of the matter must await further evidence.

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B. In the absence of certain evidence for the restriction of the function of learning to special structures we may ask whether there is any sort of nervous organization which is particularly well adapted for learning, any structures whose functional activity facilitates learning. The question is more complicated than at first appears and should perhaps be stated in a different way. Given equally strong incentives for learning and problems equally well adapted to the innate equipment of the organism, will learning take place any more rapidly because of the presence of any special type of nervous organization? The mere demonstration that animals with a complex cerebrum are capable of learning complex problems more rapidly than those at a lower evolutionary level will not answer the question. A bird will learn to fly more quickly than a man and a man to speak more rapidly than a bird, but these facts do not prove that simple conditioned reflexes are formed any more rapidly in one than in the other when the conditions of the experiment are adapted to the two organisms. The rate of acquirement of complex habits is dependent upon many factors besides the actual rate of fixation of new integrations. The preëxisting habits of the organism, the number and variety of instinctive responses available, the complexity of organization of existing reaction systems, perhaps the mere number of unemployed association fibers in the nervous system, all may influence the rate of learning, even though the underlying mechanism is the same. The phylogenetic evidence is not sufficient to prove that there has been any specialization of structure to facilitate learning.

Such experimental work as bears on the question indicates a lack of any important specialization for learning. Destruction of the frontal lobes is followed by loss of habits, yet animals lacking the frontal lobes may learn as rapidly as normal ones (Franz, '02, '07). The slow improvement in human reëducation after cerebral injury might be interpreted as proving the reverse, but reëducation demands the establishment of an enormous number of habits and the rate of improvement in the aphasic, for example, can not be compared with that of a normal adult, but only with that of the child first learning to speak. Measured

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by this standard, the progress of reëducation is not surprisingly slow. Results indicating slower learning after cerebral lesion are significant for this question only when the rate of learning is independent of preëxisting habits. Experimental work where this condition has been met covers only the frontal region of the cerebrum and must be extended to other parts as well.

C. A number of observations establish the fact that learned reactions are mediated by certain regions of the cerebrum, the frontal and parietal association areas in higher mammals, the frontal pole in the rat. Is the function of these areas truly associational, that is, do they function in the reintegration of habits as such, or do they take part in the performance of the habit simply because they lie, anatomically, between the necessary afferent and efferent projection areas? In other words, do the association areas have a directive function in learning or are they masses of conductive tissue through which nerve impulses penetrate at random from afferent to efferent projection areas in the formation of new functional connections? There is no evidence which answers this question. Franz ('07) found that destruction of the frontal region of the brain of cat and monkey abolished recently formed habits, but the habits studied all had a kinaesthetic-motor foundation and it is not clear that all habits would be abolished by this operation provided that they had a different sensory basis. The problem can be attacked effectively only by the study of the acquirement of a variety of habits after the destruction of each and all of the association areas.

D. The suggestion of complete vicarious functioning of the parts of the cerebrum in learning, raised by Lashley and Franz and extended in this paper, raises the further question of the relation of the absolute quantity of cerebral material to the rate of learning. There seems to be no evidence bearing upon this point.

E. It has been suggested by many writers that habits which are practiced until they become automatic are thereby reduced to subcortical levels. The only experimental evidence bearing upon the point that I have been able to find is the statement

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by Franz that habits of long standing persisted after operations which abolished more recently acquired ones. His experiments do not seem to have precluded the possibility that the older habits involved reactions to different stimuli (auditory and visual) from those which called out the more recently formed ones (kinaesthetic). The opinion of other writers seems to be based chiefly on the desire to hustle unconscious reactions out of the cerebrum and there is no conclusive evidence that any habit which may be performed at subcortical levels was not acquired there in the first place.

These problems are perhaps the most fundamental ones for an understanding of nervous function in learning which will serve as a foundation for investigation of the basic process of reintegration. Many others of a somewhat more limited character are raised by the facts of cerebral localization in man but the facts in that field are too complex and too poorly understood to make a discussion of them in this limited space profitable. Still other problems are raised by the results of the present investigation and will be considered after the data have been presented.

Program of experiments

The experiments reported in the present paper bear upon many of the questions raised in the foregoing discussion but they were devised especially to test the following points:

1. Does cerebral injury give rise to disturbances in the general reactions of the rat such as would interfere seriously with learning, even though the essential structures for learning were still intact (section II)?

2. Is any part of the cerebrum especially efficient in learning irrespective of the type of sensory-motor coördination involved in the habit (section III)?

3. Is there any relation between the mass of cerebral material functioning and the rate of learning (section IV)?

4. Is the frontal region, which is utilized in the formation of the inclined-plane box habit, functional in all learning, or is it utilized in this habit simply because of the particular sensory-motor coördinations involved (section V)?

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5. Does the motor area (determined by electrical stimulation and effects of destruction upon motor coördination) act as the final common path for learned reactions, or may the efferent impulses of learned reactions be transmitted from other regions (section VI)?

The methods employed were essentially those devised by Franz for the study of the frontal lobes; training the animals before or after operative destruction of cerebral areas and comparing their learning ability with that of normal animals.

Operative technique

The operations were performed under deep ether anesthesia. In every case a median longitudinal incision was made through the skin from the base of the skull to the anterior margin of the orbits, the hair having been removed by a depilatory. The skin was then drawn back to expose the entire dorsal surface of the skull. The operative field was mapped out in relation to the skull sutures and an opening of the required size was made with trephine and bone forceps. In case of bilateral operations the skull openings were made on both sides of the longitudinal suture to avoid injury to the longitudinal sinus.

The cerebral cortex was destroyed either by cauterization or by cutting it away from the underlying tissues with a cataract knife. For cauterization a small electric cautery was used, but owing to the quick cooling of its point and its inconvenient form it proved less satisfactory than the knife. In those experiments where it was used the fact is indicated. As soon as hemorrhage from the brain injury was checked the skin incision was closed by three stitches and covered with a cotton-celloidin dressing.

Since retention was tested shortly after the operation in some cases the question of irritation from the wound is of importance. In general the animals gave little evidence of discomfort after the operation. In three cases, after almost complete removal of one hemisphere, the animals ate heartily within thirty minutes after recovery from anesthesia. The positive results of the retention tests furnish the best evidence that irritation from the wound does not seriously interfere with the tests.

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Determination of the extent of the lesions

At the end of the experiments the animals were killed with ether and their brains removed, fixed in 15 per cent formalin, and imbedded in celloidin. Horizontal sections, 50μ in thickness, were cut and each fifth section was kept for study. The sections were drawn in outline under a projection microscope and the details of the lesions were filled in under higher power, free hand. Owing probably to impurity of the alcohol used for dehydration the sections proved unusually resistant to stains and it was not always possible to make out cell details in them. For that reason, only areas in which the destruction of the cortex was obvious, as by complete absorption or by absolute separation from the remainder of the brain, were recorded as lesions. The lesions recorded represent therefore the minimal extent of injury; much larger areas than those indicated were almost certainly rendered nonfunctional by the section of their subcortical connections.

From the drawings of the sections the dimensions of the lesions were transferred with proportional dividers to corresponding levels on the diagrams of the brain, the points so determined were connected by lines, and the injured areas thus plotted were finally inked in. The method permits of some error, owing to disproportion between the sections and the diagrams, but it seems accurate enough for the present purpose. In addition to the diagrams of the dorsal and lateral aspects of each injured hemisphere a detailed camera drawing of one section was prepared. This was selected in the plane of the thalamus and lateral ventricles wherever possible. Because of limited space for reproduction these figures of sections had to be made somewhat diagrammatic. The degenerated cortex is indicated on them in solid black (plates 1, 2, and 3).

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II. THE RELATION OF CEREBRAL INJURY TO THE GENERAL ACTIVITY OF THE RAT

Various disturbances in the general activity of animals suffering from cerebral lesions have been described, such as restlessness in frontal cases or inertia and loss of emotional reactions after occipital injury. Although such changes have not been worked out carefully enough so that we may predict their occurrence after any given operation the possibility of their existence in operated animals complicates the study of the effects of cerebral lesions upon learning. Measures of learning ability of partially decerebrate animals based upon the rate of formation of a motor habit may be deceptive owing merely to differences in the general activity of the animals. The complicated problems which are adapted to the study of habit-formation in animals all demand the solution of some problem by overt trial and error and the fixation of certain parts of the reaction in habit. Theoretically, every animal must solve the problem at every trial so that differences in the amount of activity will make no difference in the amount of practice expressed in a given number of trials. But other factors besides frequency are instrumental in determining the rate of learning and among these the temporal element is of fundamental importance. The slow or inactive rat will require a longer time per trial than will the active one. Perhaps he will thus gain the advantage of distributed practice and thus learn with less effort; perhaps delays between the acts of the series to be associated will prolong the process of association as they do in the formation of the conditioned reflex. We have no means of determining, at present, what the real influence of such conditions will be. It becomes necessary, therefore, to control the general activity of the animals carefully, or where this is impossible, to determine the existence of individual variations in general activity so as to avoid the mistake of ascribing to specific cerebral injuries effects which are due merely to general ill health, or of interpreting slow learning as due to loss of problem-solving ability or to slowed rate of reintegration when its

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cause is in reality the loss of reinforcing or inhibiting mechanisms which determine only the tonic condition of the animal.

Before the results of training could be interpreted it was necessary, therefore, to make some estimate of the relative activity of normal and of partially decerebrate animals. Observations during training indicated that the normal animals were more given to excess activities, such as leaping over low obstacles (see page 92), than were the operated ones, but aside from this "friskiness" the normals seemed in no way more active than the operated animals. The latter spent little time in complete inactivity, moved quickly about the restraining cages, and reacted to stimuli as promptly as did normals. There was no indication of differences in general activity which might influence the rate of learning.

Methods

For a more accurate measure of the general activity of the groups whose rates of learning were compared, continuous records of activity for 240 hours were made for some of the animals trained and for other normal ones. These records were obtained by the use of a modification of the apparatus designed by Slonaker ('08). Four revolving cages were arranged so that their movements were recorded on a disk kymograph giving continuous records over twenty-four hour periods. A sample from such a record, covering six hours, is shown in figure 1. The cages were pivoted so that the slightest movement of the animal within them would produce a break in the continuous line of the record sheet. The figure shows periods of continuous activity with rapid spinning of the cage (*a*), periods of practically complete quiescence (*b*), and periods of slight activity, restlessness without active running (*c*). Observations on the animals indicate that the periods of nearly complete inactivity are spent in sleep. Periods of partial activity are difficult to interpret and in dealing with the records statistically it seemed best to include only periods of active running and periods of complete quiet. The active running was computed as average

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hours per day during which the cage revolved almost continuously. Thus about 50 per cent of the hour at *d* was spent in running and this was added to all other periods of great activity for the day. No period of inactivity was counted unless it was a full hour in length and the total inactivity is expressed as the average number of hours per day in which there was no activity. These rather arbitrary methods were selected because they set an absolute standard which may be recognized easily and is not subject to the errors which would arise if we attempted to estimate the total activity represented by such a curve as that at *c*. As an absolute measure of activity this method is

FIG. 1. A RECORD OF THE ACTIVITY OF FOUR RATS FROM 8 P.M. TO 2 A.M.
a, Almost continuous running; *b*, complete rest; *c*, partial activity.

obviously inaccurate, but since the records of all the animals were treated in the same way the method of analysis is valid for comparative work.

Results

The determinations of general activity were made with normal animals and with individuals having each of the lesions dealt with in the training experiments. Their records for periods of great activity and for periods of inactivity are shown in table 1. As the records stand they indicate that the parietal cases were least given to active running, though not more than normally

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quiet, that the occipital and hemidecerebrate cases were slightly less active than normals, that the animals with frontal lesions were much more active than normals, and that those with frontal and parietal lesions were both more active and more restless than any of the others.

TABLE 1

Duration of periods of great activity and of complete quiescence in normal animals and in animals after cerebral injury. The figures are based upon continuous records over 240 hours for each animal. The total number of hours per day spent in active running is given in the first part of the table. The total number of hours per day in which there was little or no activity is given in the second part. The numbers in parenthesis are those assigned to the animals in later experiments (section III)

	Position of lesion					
	Normal	Hemidecerebrate	Occipital	Parietal	Frontal	Fronto-parietal
Total activity....	5.82 2.55 8.82 4.31 6.40	2.67 (1) 6.32 (4) 5.19 (5)	0.92 (7) 7.36 (8) 4.85 (9)	3.73 (11) 3.76 (12) 3.50 (13)	5.34 9.33 8.90	7.02 (18) 9.66 (19)
Average.....	5.58	4.72	4.35	3.66	7.86	8.34
Total rest.....	7.1 4.9 3.6 8.6 6.5	10.3 (1) 6.5 (4) 5.8 (5)	3.4 (7) 7.0 (8) 9.5 (9)	5.5 (11) 7.1 (12) 12.6 (13)	9.9 6.4 4.3	3.0 (18) 2.6 (19)
Average.....	6.14	7.50	6.63	8.4	6.8	2.8
Average trials required for learning		96.0	83.0	74.3		39.0

The apparently greater activity of the frontal cases might be explained by the fact that these animals were somewhat younger than the others rather than as due to the lesion but the assumption is not borne out by the behavior of the fronto-parietal cases which were much above the average in age. It seems rather that the heightened activity is the result of the cerebral injury, particularly as many other observers have reported

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increased restlessness after frontal injury in higher animals. The animals of each group are arranged in the same order in the two parts of the table so that their records for activity and rest may be compared. Usually a great amount of active running is accompanied by a reduced amount of time spent in absolute inactivity, but there are marked exceptions to this rule.

There is a great amount of individual variation between the members of each of the groups and there seems to be no constant relation between the amount of activity of the animals and the number of trials required by them for learning the problem on which they were trained. Three animals which showed little activity (numbers 1, 7, and 11) required many trials for learning (141, 107, and 101 respectively), but one very inactive animal (13) learned in only 41 trials. Two very active animals (18 and 19) required few trials (51 and 27 respectively), but an equally active animal (8) required 97 trials for learning. There is no absolute correspondence between the general activity and the rate of learning.

The small number of animals studied and the great amount of individual variation makes the significance of differences between the groups very questionable. The test seems to have brought out the fact, however, that cerebral injury has no constant effect upon general activity which could account for any great differences in rate of learning found between normal and partially decerebrate animals.

A second question respecting the general activities of the operated animals is of great importance in interpretation of the results of training. Does destruction of parts of the cerebrum so disturb the instinctive behavior of the animals as to modify the effects of the stimuli used as incentives to learning? I have kept operated animals for periods up to five months after operation and have not been able to discover any change in their instinctive reactions. Animals of every group showed sexual activities, exploration of new situations, great eagerness for food after periods of starvation, efforts to be the bottom one of the pile when several animals slept together in one cage, etc. Except for motor incoördination which appeared in a few

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animals it would have been impossible to distinguish the operated animals from normals on the basis of any instinctive reaction, when once the immediate effects of the operation had worn off. (That the lack of motor coördination may lead to mistaken interpretation of other behavior has already been pointed out.) There is no evidence for any disturbance in instinctive reactions which might account for marked difference in learning ability.

III. THE EFFECT OF CEREBRAL INJURY UPON LEARNING ABILITY

Problem and methods

The first group of training experiments sought to measure the effect upon learning ability of the removal of various portions of the cerebral cortex, to determine whether or not any particular part of the cerebrum is necessary or is especially well adapted to the formation of complex habits. The method adopted was that of destroying a definite area of the cortex in each of a number of animals, varying the operations so as to cover all regions of the cortex in the total series of experiments; then to train the operated animals and compare their rates and methods of learning with those of normal animals.

Since there is a possibility that some simple habits are acquired wholly at subcortical levels (Franz and Lashley, '17) it seemed desirable to use a complicated problem for training. A second requirement of the problem to be learned was that it should demand some definite series of acts which could be recognized easily and which would be unlikely to appear as the result of pure chance, since the same problem was to be used for learning and for retention tests and time and error criteria alone are not very reliable tests of retention when applied to animals suffering from shock and motor disturbances following operation. A third requirement of the problem was that it should offer equal chances for learning to normal animals and to those which were weak as the result of operation. The inclined plane box with the plane situated above the door, used previously by Lashley and Franz, was not very satisfactory, since the operated animals sometimes had difficulty in climbing up to the plane.

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After some preliminary tests a combination-latch box was constructed which seemed to meet the above requirements. Its general plan is shown in figure 2. It consisted of the usual square, wire-covered box with a door (*d*) which opened inward by the pressure of a spring. The door was held closed by latches attached to two metal platforms (*a* and *b*) at opposite ends of the box. The platforms were 10 cm. long by 5 cm. wide and

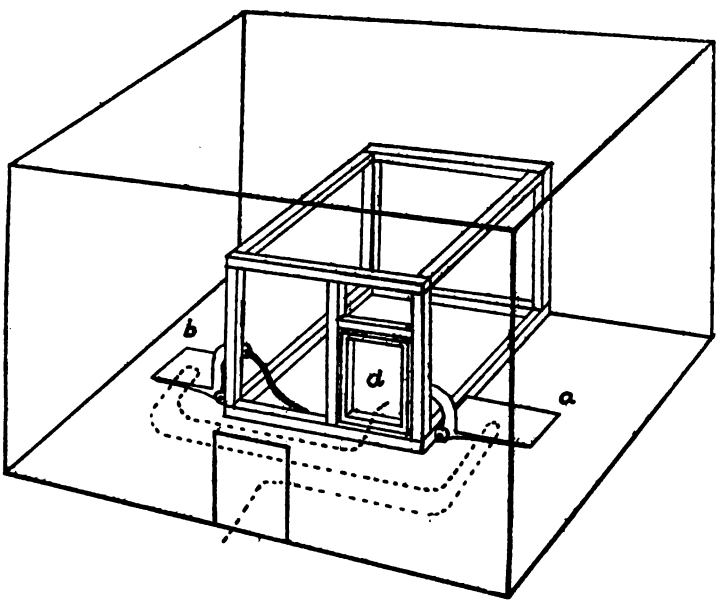

FIG. 2. THE DOUBLE-PLATFORM BOX

a and *b*, platforms which must be pushed down in the order *a*–*b* in order to open the door, *d*, to the food box.

2 cm. above the floor. Their inner ends were hinged to the sides of the box and arranged to release the latches when their outer ends were depressed. By adjustable springs under the platforms the weight required to depress them could be regulated. Throughout the experiments they were adjusted so that a weight of 50 grams would just release the latches. The door was placed in the side of the box near one end. Pressure on the platform at that end (*a*) released one latch and allowed

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the door to move inward against the second latch. In this position of the door the first latch could not catch again. Pressure on the second platform (*b*) would now release the second latch and allow the door to open.

As a result of this arrangement the door could be opened only by depressing the platforms in succession, beginning with the one nearest the door (*a*). Pressure on the first platform (*a*) caused only a faint click, pressure on the second platform (*b*) caused the door to open, but only after the first platform had been depressed.

Food was placed in the box and this was enclosed in a restraining cage which allowed a space of about 30 cm. around the sides and top of the box. The animals were admitted through a small door in the restraining cage, opposite the door of the problem box. A stop-watch was started as soon as the animals entered the restraining cage. The times required to trip the first platform, to trip the second platform, and to enter the box were recorded. If the animal failed to open the box in an hour, except on the first trial, he was taken out and not fed. On the following day the latches were set as he had left them and the trial was continued. On the first trial inactive rats were allowed to remain in the restraining cage over night if necessary. Since none of the animals was fed in the problem box until after the first trial, the time of the first trial is significant only as a measure of general exploratory activity and it is not included in the comparison of times required for learning.

As it was not possible to breed rats in the laboratory in time for the work, the ancestry and exact age of the animals used in the experiments is unknown. All were young when purchased, however, and their age could be estimated roughly. All were between 120 and 180 days old at the time of operation, with the exception of numbers 1 and 17 which were older. Training was begun at intervals of from 13 to 43 days after the operation.

As a measure of learning the following arbitrary criterion was employed. The animal must go directly from the door of the restraining cage to plane *a* and push down the platform; he must then go by some constant path to plane *b* and after pushing it down must go directly to the open door and enter the problem

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box. Any deviation from the direct path or pause to sniff at the door or other part of the problem box was counted as an error. Three trials were given daily and three successive trials without error must be made in one day before learning was considered perfect.

Experimental data

Five types of operations were used for comparison and an attempt was made to test several animals with each type of lesion. The groups covered by the experiments were the following:

Group 1. Normal animals	10
Group 2. One hemisphere removed	6
Group 3. Occipital regions of both hemispheres destroyed	4
Group 4. Parietal regions of both hemispheres destroyed	5
Group 5. Frontal poles of both hemispheres destroyed	2*
Group 6. Frontal and parietal regions of both hemispheres destroyed	2*

* The experiments of Lashley and Franz cover these regions and the training of these additional animals was undertaken only as a check on the earlier work.

The technique used in training all the groups was the same and there is no reason to believe that they differed in any other respect than the amount of functional cerebral cortex. The records of individual experiments follow.

Group 1. Normal animals. The ten normal animals were trained before the operative experiments were begun, as controls in an experiment on the action of drugs. Except for the season during which training was carried out, they did not differ from the operated animals, and the difference even here was slight, since training of the latter was begun before the normals had completely learned their problem. The records of the animals of this group are given in table 2, together with the records of the other groups, and their average time per trial in successive day's practice is shown in table 3.

The average number of trials required for learning by members of the group was 142.6 ± 9.2.

Group 2. Hemidecerebrate animals. In this group the object was to destroy one hemisphere completely while leaving the other intact. Complete destruction was obtained in only one case,

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TABLE 2

The average numbers of trials and range of variation in learning the double-platform box by normal and operated groups

GROUP	LESION	AVERAGE TRIALS	RANGE OF VARIATION
1	Normal	142.6	63–204
2	Hemidecerebrate	87.2	49–141
3	Occipital injury	68.8	45–107
4	Parietal injury	80.0	41–101
5	Frontal injury	90.0	90
6	Fronto-parietal	39.0	27–51
	All operated	79.0	27–141

TABLE 3

The average time in seconds per trial, based on the average of each successive nine trials, required by the animals in each of the groups studied to go from the entrance of the restraining cage to the food

NUMBER OF TRIAL	GROUP NUMBER					
	1	2	3	4	5	6
2–9	429.5	173.9	330.2	1670.0	384.5	475.6
–18	132.2	81.2	61.5	95.4	116.2	87.5
–27	47.1	50.2	38.8	47.0	73.4	69.0
–36	40.5	33.8	39.3	26.6	29.8	12.6
–45	22.4	25.1	33.7	26.0	40.5	22.2
–54	13.7	22.2	29.8	25.9	56.9	10.5
–63	12.8	19.1	28.8	33.0	41.6	
–72	11.3	18.7	23.9	25.8	37.6	
–81	12.0	16.5	41.7	17.3	26.2	
–90	14.7	14.9	24.9	13.5	14.1	
–99	13.9	13.6	11.9	10.6	9.0	
–108	12.1	13.7	9.0	6.2		
–117	10.6	14.3				
–126	10.1	22.9				
–135	10.4	14.0				
–144	8.6	12.2				
–153	9.4	11.0				
–162	8.4					
–171	9.1					
–180	8.2					
–189	6.6					
–198	7.4					

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but in all of the others a very large proportion of the hemisphere was destroyed. Brief descriptions of the training and extent of the injuries in each of the animals follow.

Number 1. Large male. Left hemisphere injured by transverse and longitudinal incisions. Recovery was very rapid without discoverable sensory or motor disturbance. Training was begun 20 days after the operation. The reactions were prompt from the first and habitual reactions to the door of the problem box appeared on the fifth trial.

Number of trials required for learning	141
Per cent of total cortex destroyed	27

Lesion: Plate I, figure 1. The entire frontal pole of the left hemisphere cephalad to the corpus striatum was completely destroyed by a section extending into the olfactory tract. On the dorsal surface the lesion extended caudad to the posterior margin of the hippocampus, involving all the cortex overlying the corpus callosum. The cortex overlying the corpus striatum and the orbital surface remained intact. Behind this the lesion extended laterad to involve all the cortex overlying the hippocampus to the level of the posterior commissure. The underlying structures were practically uninjured.

Number 2. Large male. Left hemisphere injured by tranverse and longitudinal incisions, with an attempt to preserve the corpus striatum. Recovery was rapid without sensory or motor disturbance. Training was begun 20 days after the operation. The behavior of the animal seemed quite normal.

Number of trials required for learning	132
Per cent of total cortex destroyed	40.7

Lesion: Plate I, figure 2. The entire cortex of the left hemisphere, cephalad to the posterior margin of the lobus hippocampus had been separated from the underlying structures and was reduced to a thin membrane. A small area on the occipital pole and the cerebellar surface alone remained intact. The olfactory lobe was completely severed. The subcortical nuclei remained uninjured.

Number 3. Small male. Left hemisphere injured by deep transverse and longitudinal incisions. He recovered somewhat slowly and showed a pronounced hemiparesis with rotation to the right in walking. The left vibrissae seemed insensitive and the behavior of the animal in

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walking with his left feet near the edge of a table suggested that of Vincent's blind rats after removal of the vibrissae. Training was begun 41 days after the operation. The animal was normally active, but at first failed to restrict his reactions to the problem box, as most normal animals do. Nevertheless, he learned with unusual quickness.

The brain was preserved 70 days after operation.

Number of trials required for learning	54
Per cent of total cortex destroyed	50

The brain was removed and fixed 91 days after operation.

Lesion: Plate I, figure 3. Practically all of the left hemisphere was destroyed, leaving only a thin membranous sac containing remnants of the corpus striatum and hippocampus surrounding the thalamus.

Number 4. Medium sized male. Right hemisphere injured by transverse and longitudinal incisions. He developed a slight paresis of the left legs with a possible reduction in the sensitivity of the left face. Training was begun 40 days after operation. The animal learned to go quickly to the platforms in the correct order, but usually merely sniffed at them and then ran to the door.

Number of trials required for learning	78
Per cent of total cortex destroyed	42.4

The brain was removed and fixed 90 days after the operation.

Lesion: Plate I, figure 4. The frontal pole of the right hemisphere was completely severed by a section passing just in front of the forceps of the corpus callosum. The dorsal surface of the hemisphere was destroyed except for a small area above the corpus striatum and another on the mesial surface of the occipital pole. On the lateral surface a section extended from the lateral ventricle through the posterior margin of the corpus striatum to the surface of the cortex, and the entire temporal and occipital regions caudad to this were degenerated and filled by a large cyst. The hippocampal lobe seemed to be uninjured, although compressed by the cyst. The posterior part of the corpus striatum was destroyed.

Number 5. Medium sized male. The left hemisphere was injured. He developed a marked right hemiparesis. Training was begun 40 days after the operation. His reactions to the problem box were normal.

Number of trials required for learning	72
Per cent of the total cortex destroyed	26.6

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The brain was removed and fixed 78 days after the operation.

Lesion: Plate I, figure 5. The lesion was practically identical in form with that of number 4, but less extensive. The frontal pole was largely destroyed by a section passing from the lateral ventricle outward along the forceps of the callosum to the cortex. The cortex laterad to the corpus striatum was not degenerated, but its connections were almost certainly destroyed. This area is stippled in the diagram. The cortex laterad to the hippocampus was completely degenerated. The dorsal surface of the hemisphere was destroyed except for a narrow band along the mesial and occipital borders. The corpus striatum was intact but seemed to be separated from its posterior attachments.

Number 6. Small female, 68 days old at the time of operation. The left hemisphere was partly destroyed by a thermo-cautery. She developed a slight paresis which persisted for only a few weeks. Training was begun 30 days after the operation. After 8 days the animal became ill and training was suspended for 12 days. It was then resumed and completed.

Number of trials required for learning	49
Per cent of total cortex destroyed	30.4

The brain was removed and fixed 60 days after operation.

Lesion: Plate I, figure 6. All the cortex on the dorsal surface from the forceps of the corpus callosum to the posterior margin of the hippocampus was destroyed. The lesion narrowed laterally to include only the area overlying the corpus striatum and fornix. The latter bodies were completely destroyed.

The results of these tests are summarized in table 2. The maximum number of trials required by any member of the group was 141 (1). The smallest number was 54 (3). The average number of trials is 82.7. The average time required per trial on successive day's training is shown in table 3. The total extent of the lesions is shown in figure 3. Every portion of the left hemisphere and the greater part of the right was eliminated in one or another test, without significantly increasing the amount of practice required for learning. The maximum lesion appeared in number 3 (plate I, figure 3), including practically the entire left hemisphere. This animal learned more quickly than any of the others.

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Group 3. Occipital regions destroyed. In these tests the attempt was made to destroy the posterior third of the cortex of both hemispheres. Autopsies showed from 18 to 40 per cent of the cortex destroyed. Accounts of individual experiments follow.

Number 7. Large female, about 100 days old. The occipital lobes were destroyed by passing a scalpel through two trephine openings about 5 mm. in front of the cerebro-cerebellar junction. Recovery was rapid without observable sensory or motor disturbance. Training was begun 27 days after the operation. The first three trials were made in very slow time, but thereafter reactions were prompt. Platform a was learned much more quickly than platform b; a marked exception to the usual rule.

FIG. 3. THE TOTAL EXTENT OF THE LESIONS IN ANIMALS OF GROUP 2 AFTER OPERATIONS ON ONE HEMISPHERE

Number of trials required for learning	107
Per cent of total cortex destroyed	32.1

Lesion: Plate I, figure 7. Both occipital regions were completely destroyed above and caudad to the hippocampal lobes, except for a small area on the mesial surface of the left hemisphere at the level of the thalamus. The right hippocampal lobe was degenerated and the external capsules in the occipital lobes of both hemispheres were filled by large cysts.

Number 8. Large, vigorous male, about 120 days old. The occipital regions were destroyed with a thermo-cautery inserted through two trephine openings situated as in the foregoing case. Recovery was normal. Training was begun 18 days after operation. His reactions were prompt from the first: the position of the door was learned by the tenth trial.

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Number of trials required for learning	97
Per cent of total cortex destroyed	17.8

Lesion: Plate I, figure 8. The lesions of both hemispheres were confined to the dorsal surface. The cerebellar surfaces of the cerebrum were intact. The hippocampal lobes were uninjured. The lesions extended laterad over the surfaces of the hippocampal lobes to the level of the fourth ventricle.

Number 9. Large male, about 200 days old. Both occipital lobes were injured with the cautery as above. Recovery was normal. Training was begun 22 days after the operation. The animal seemed quite normal in behavior, was aggressive and dominated the other males in the cage with him. In the problem box his reactions were prompt and the door was learned in six trails.

Number of trials required for learning	45
Per cent of total cortex destroyed	40.8

Lesion: Plate I, figure 9. This animal showed the most extensive destruction of any of the occipital cases. The lesions of both hemispheres extended from the anterior edges of the hippocampal lobes caudad and laterad to include all of the cortex dorsad and laterad to the hippocampal lobes and all the posterior mesial surfaces except a small area on the left hemisphere at the level of the thalamus. The hippocampal lobes seemed intact.

Number 10. Small male, 102 days old. The occipital lobes were injured with a scalpel. Recovery was rapid with no noticeable disturbances of behavior. Training was begun 13 days after operation, before the animal had recovered his full strength. For the first two trials he was allowed to remain in the problem box over night. Thereafter his reactions were prompt.

Number of trials required for learning	82
Per cent of total cortex destroyed	20.3

Lesion: Plate I, figure 10. On the right hemisphere the lesion extended over an area covering the dorsal and lateral surfaces of the hippocampal lobe, narrowing as it passed laterad and ending at the level of the fifth ventricle. The dorsal surface of the lobus hippocampus was destroyed. The lesion on the left hemisphere was similar but less extensive.

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The results of these tests are summarized in table 2. The maximum number of trials required by any member of the group was 107 (7). The minimum number was 45 (9). The average number is 68.8.

The total extent of the lesions is shown in figure 4. Every part of the occipital third of the cortex was destroyed in some one of the animals without any significant increase in the amount of practice required for learning. The average time per trial required in successive day's practice is shown in table 3.

The greatest lesion appeared in number 9 (plate I, figure 9) and this animal learned more quickly than any of the others.

Group 4. Parietal region destroyed. In these experiments the attempt was made to destroy the middle third of the cortex, an

FIG. 4. THE TOTAL EXTENT OF THE LESIONS IN ANIMALS OF GROUP 3 AFTER OPERATIONS ON THE OCCIPITAL POLE

area including the motor cortex of the hind legs and extending caudad to the visual cortex. Autopsy showed in each case a saddle shaped area, including from 14 to 31 per cent of the cortex, destroyed. Accounts of the individual experiments follow.

Number 11. Small female, about 150 days old. The parietal regions were injured by passing a scalpel laterad and messiad from each of two trephine openings just caudad to the fronto-parietal suture. Recovery was uneventful with no abnormalities of behavior. Training was begun 25 days after operation. The animal was inactive at first, but aften ten trials her reactions to the problem box became normal.

Number of trials required for learning	101
Per cent of total cortex destroyed	21.2

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Lesion: Plate II, figure 11. The brain could not be sectioned owing to the presence of a large piece of bone which had become imbedded in the right hemisphere, extending into the lateral ventricle. The black area in the diagram indicates the extent of the large cyst occupying the parietal lobes of both hemispheres, as determined by gross dissection. The cortex in this area was completely absorbed, so that the thalamus and lateral ventricles were exposed. All of the cortex and that part of the corpus callosum overlying the lateral ventricals was destroyed.

Number 12. Small female, about 150 days old. Both parietal lobes were injured by an operation similar to that performed on number 11. Recovery was normal without sensory or motor disturbance. Training was begun 27 days after operation and progressed normally.

Number of trials required for learning	81
Per cent of total cortex destroyed	15.2

Lesion: Plate II, figure 12. The lesion extends caudad from the region of the fronto-parietal suture to the anterior border of the hippocampus and laterad to the level of the floor of the lateral ventricles, involving the middle portion of the corpus callosum.

Number 13. Small male, about 100 days old. The parietal lobes were injured as in the case of number 11. Recovery was rapid and training was begun 18 days after operation. The animal's reactions were prompt from the first and selective reactions to the platforms appeared on the fourth trial.

Number of trials required for learning	41
Per cent of total cortex destroyed	31.5

Lesion: Plate II, figure 13. On the right hemisphere the lesion extends from the knee of the corpus callosum caudad to the middle of the hippocampal lobe. Laterad it extends to the orbital surface and is continued as two narrow cuts which extend almost to the olfactory tracts and are united internally by a section through the external capsule which is outlined by small cysts. The lesion of the left hemisphere is similar in position but extends caudad only to the anterior margin of the hippocampus and laterad only to the plane of the upper limit of the lateral ventricle.

Number 14. Small male, about 100 days old. The parietal lobes were injured with a thermo-cautery. Recovery was rapid, but a slight right paresis persisted. Training was begun 25 days after operation. He reacted promptly to the problem-box situation but soon developed

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a stereotyped reaction to the second platform which delayed learning considerably.

Number of trials required for learning	99
Per cent of total cortex destroyed	24.3

Lesion: Plate II, figure 14. On the dorsal surface of both hemispheres the lesion extended from the knee of the corpus callosum to the middle of the hippocampal lobes. On the right hemisphere it narrowed laterad, extending over the surface of the hippocampus. On the left hemisphere the lesion involved the same areas and in addition most of the cortex overlying the corpus striatum was destroyed. The lower nuclei seemed to be uninjured.

Number 15. Small female, 70 days old. The parietal areas were cauterized as in the case of number 14. Recovery was slow and the

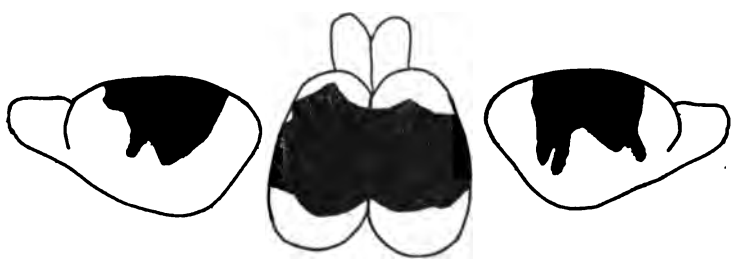

FIG. 5. THE TOTAL EXTENT OF THE LESIONS IN ANIMALS OF GROUP 4 AFTER OPERATIONS IN THE PARIETAL REGION

animal was still weak at the beginning of training. Reactions were prompt from the first and learning progressed normally.

Number of trials required for learning	78
Per cent of total cortex destroyed	14.2

Lesion: Plate II, figure 15. The lesions on both hemispheres were confined to small areas lying above the lateral ventricals and corpora striata. Subcortical structures were uninjured.

The results of these tests are summarized in table 2. The maximum number of trials required by any member of the group was 101 (11). The smallest number was 41 (13). The average number of trials is 80. The average time required per trial on successive days' training is shown in table 3.

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The total extent of the lesions is shown in figure 5. The entire middle third of the dorsal surface of the cerebrum was covered. The injuries on the lateral surfaces do not include all of the temporal lobes. No one of the animals of this group required as many trials for learning as the average of normals.

The greatest extent of injury appeared in number 13 (plate II, figure 13) and this animal learned more quickly than any of the others.

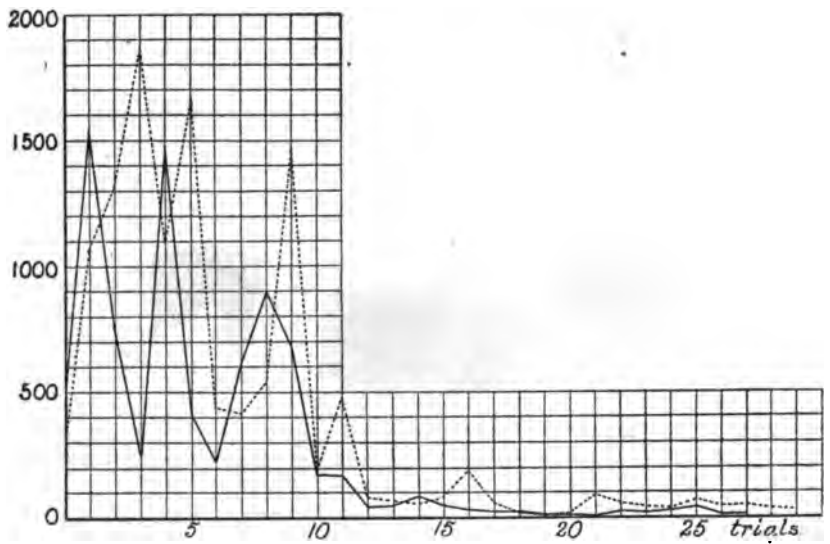

FIG. 6. A COMPARISON OF THE RATE OF LEARNING IN NORMAL ANIMALS AND IN ANIMALS AFTER FRONTAL OPERATIONS

The average time in seconds per trial is plotted. Normal rats: - - - -, Operated rats ———. From Lashley and Franz, '17.

Group 5. Frontal pole destroyed. In earlier work this area was covered rather thoroughly by experiments on the inclined-plane box. In that work six animals were trained on the inclined-plane box after injury to the frontal pole of the cortex and their average rate of learning was compared with that of normals on the same problem. The curves showing the rates of learning of these two groups are reproduced in figure 6. The total extent of the lesion in the six animals is shown in figure 7. There was no significant difference between these groups (Lashley and Franz, '17).

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For comparison with this work two animals have been trained on the more complicated double-platform box after destruction of the frontal poles of the cerebrum. Brief descriptions of their records follow.

Number 16. Large female, about 200 days old. The frontal lobes were destroyed by transverse sections from trephine openings just back of the fronto-parietal suture. Recovery was normal without behavior disturbances. Training was begun 43 days after operation. Learning progressed normally.

Number of trials required for learning	90
Per cent of total cortex destroyed	15.5

Lesion: Plate II, figure 16 (17). Both frontal poles were completely severed from the remaining cortex by a transverse section extending

FIG. 7. THE TOTAL EXTENT OF THE LESIONS IN ANIMALS WHOSE RECORDS ARE SHOWN IN FIGURE 6

from a point above the knee of the corpus callosum diagonally cephalad to the olfactory tracts. The cerebral nuclei were uninjured.

Number 17. Large female, about 200 days old. The operation was similar to that of number 16. The animal recovered rather slowly and remained emaciated throughout the experiment. Training was begun 43 days after the operation. Reactions were slow and deliberate. The animal gave evidence of rather rapid learning during the first 40 trials, then developed a stereotyped reaction to the second platform, making sometimes as many as 100 trips to this before going to the first. After 200 trials she made a number of correct reactions, but never succeeded in meeting the requirements of the problem. From the 50th trial on she gave evidence of illness, and by the 250th trial she had become so weak that it seemed best to drop training.

Number of trials required for learning	250+
Per cent of total cortex destroyed	15.5

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Lesion: Plate II, figure 16 (17). The lesion was identical in every respect with that of number 16, except that the olfactory tracts were severed, and as the figures for the two brains were practically indistinguishable, only that for number 16 is included in the plate.

One of these two animals learned in normal time, the other failed to learn the problem. The latter is particularly interesting since she is the only animal of the series that failed. Her brain was examined with especial care, but no injury could be found that would account for the inability to learn the problem. The section of the olfactory tracts will not account for the failure, since, as was brought out in the protocol, she learned the second platform very quickly. In all other respects the lesion was so nearly identical with that of number 16, that the sections could scarcely be distinguished. The animal was ill throughout the experiment and this fact probably accounts for her behavior.

Number 16 learned in normal time after destruction of 15.5 per cent of the cortex, in the region of the frontal pole. The object of the tests was simply to check up the previous observations and a single positive case of learning is sufficient for this. The double-platform box, as well as the simpler inclined-plane box may be learned in the complete absence of the frontal pole of the cerebrum.

Group 6. Frontal and parietal areas destroyed. The object of these operations was to destroy a very large area in the frontal region, including all of the stimulable area. In this respect they were successful, although the lesions were less extensive than I had intended. They include from 24 to 31 per cent of the entire cortex. Descriptions of individual tests follow.

Number 18. Small male, about 150 days old. Large openings were made in the skull over the parietal regions and an attempt was made to destroy all of the frontal and parietal area through these. The animal recovered rapidly, but was very wild and savage for some time after training was begun, 38 days after operation.

Number of trials required for learning	51
Per cent of total cortex destroyed	24.9

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Lesion: Plate II, figure 18. On the right hemisphere a transverse cut completely separated from the rest of the brain all of the cortex lying above the corpus callosum, from the anterior border of the hippocampus to the olfactory fibers at the base of the frontal pole. The latter are uninjured, as are the subcortical nuclei. The lesion of the left hemisphere is similar to that on the right, except that it extends caudad to the middle of the hippocampal lobe.

Number 19. Medium sized female, about 150 days old. The operation was similar to that in the case of number 18. The animal developed a slight right paresis which persisted throughout the experiments. Training was begun 22 days after operation.

Number of trials required for learning..... 27
Per cent of total cortex destroyed..... 31.9

FIG. 8. THE TOTAL EXTENT OF THE LESIONS IN ANIMALS OF GROUP 6 AFTER OPERATIONS ON THE FRONTAL AND PARIETAL REGIONS

Lesion: Plate II, figure 19. On the right hemisphere the lesion involved the cortex overlying the corpus striatum and the lateral surface of the frontal pole, to the level of the lateral ventricle. On the left, the lesion included the cortex overlying the corpus striatum, and the latter was also destroyed. The remaining cortex was not degenerated, but the section clearly passed entirely across the frontal pole through the corpora striata and the septum lucidum, and the inclusion in the lesion of all the cortex cephalad to this seems justified.

The records of these animals are included in table 2. The greater number of trials required is 51 (18), the lesser number is 27 (19). The average is 39. The average time required per trial on successive days' training is shown in table 3.

The total extent of the lesions is shown in figure 8. The entire stimulable area and the entire frontal pole were eliminated with-

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out reducing the learning ability of the animals. The greater lesion appeared in number 19 (plate II, figure 19) and this animal required the fewer trials for learning.

Summary of data

The tests of the learning ability of animals after destruction of areas of the cerebrum cover practically every portion of the cerebral cortex. If we include group 2, every portion is covered but since it might be urged that in hemidecerebrate cases the remaining hemisphere functions normally it seems best to treat of this group separately. The total destruction in all cases except group 2 and number 17 (animal which failed) is shown in figure 9. In these cases, where the destruction was bilateral and nearly

FIG. 9. THE TOTAL EXTENT OF DESTRUCTION OF ALL ANIMALS TRAINED ON THE DOUBLE-PLATFORM BOX, EXCLUSIVE OF UNILATERAL OPERATIONS

symmetrical, all of the frontal, dorsal, and occipital region has been destroyed and there remains only a small basilar region which has not yet been explored. I have made many attempts to produce injuries restricted to this area, both by dissection and by injection of chemicals, but the operations have always been attended by hemorrhage around the medulla which resulted fatally. However, the region is thoroughly covered by one of the hemidecerebrate cases and this, with the results which have attended operations on other portions of the cerebrum make it practically certain that this unexplored area is of no greater importance for learning than is any other part of the cerebrum.

The numbers of trials required for learning and the extents of injury to the cerebrum in each animal studied in these experi-

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ments are summarized in table 4. The average numbers of trials required for learning by the members of each group are included in table 2. From these two tables it is evident that the operated animals were not at all inferior to normal ones trained under the same conditions. Indeed, as the data stand, it seems that the operated animals are much superior to the normal controls. The average number of trials required by the normal ani-

TABLE 4

The number of trials required by operated animals for learning the double-platform box, with the percentage of the total cerebral cortex injured

NUMBER	TRIALS	PER CENT INJURY	LESION	BEHAVIOR
1	141	27.0	Hemidecerebrate	Normal
2	132	40.7	Hemidecerebrate	Normal
3	54	50.0	Hemidecerebrate	Hemiparesis
4	75	42.4	Hemidecerebrate	Slight left paresis
5	72	26.6	Hemidecerebrate	Marked right paresis
6	49	30.4	Hemidecerebrate	Slight right paresis
7	107	32.1	Occipital injury	Normal
8	97	17.8	Occipital injury	Normal
9	45	40.8	Occipital injury	Cage bully
10	82	20.3	Occipital injury	Normal
11	101	21.2	Parietal injury	Normal
12	81	15.2	Parietal injury	Normal
13	41	31.5	Parietal injury	Normal
14	99	29.3	Parietal injury	Slight right paresis
15	78	14.2	Parietal injury	Normal
16	90	15.5	Frontal injury	Normal
17	250	15.5	Frontal injury	Ill
18	51	24.9	Entire motor area	Wild
19	27	31.9	Entire motor area	Right hemiparesis

mals is 142.6. The average number required by all operated animals (exclusive of number 17) is 79. The difference is 63.6 trials in favor of the group with injury to the cerebrum, or 44.6 per cent.

In this respect the experiment seems to prove too much! Had such a difference appeared in the other direction we might have been inclined to accept the result without question as indicating a significant retarding effect of cerebral injury upon the learn-

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ing process. But we can not accept as readily the view that cerebral injury results in an improvement in learning ability. What, then, is the cause of the difference between the normal and operated groups? The first explanation suggested is that the problem box used in some way favored the injured animals.

Observation of the animals during training indicates that the double-platform box does so favor the operated animals. When the normal rat encounters a low obstacle in his path he is very apt to jump over it. In some preliminary tests the platforms used were only 2 cm. broad. Normal animals failed to learn the problem in this form because they almost invariably jumped over the platforms and so failed to trip the latches. Platforms 5 cm. in width were then substituted for the narrower ones and with this modification the normal animals were able to learn the problem, although they still frequently leaped over the platforms. During the experiments with the operated animals I gained the impression that they leaped the platforms less frequently than normal animals, but did not collect statistics on the point. A crucial test of it is furnished, however, by a comparison of the learning rates of paretic animals with those of the remainder which showed no motor disturbance. The averages of paretic and non-paretic animals are the following:

	Trials for learning
Paretic animals required	62.6
Non-paretic animals required	87.1

From this it appears that a definite paresis is advantageous for learning the problem. Animals with motor disturbance are more apt to trip the catches by chance than those without, and they therefore have a better chance to learn. A part of the apparent superiority of the operated animals may thus be ascribed with certainty to a lack of vigor. But how much of the total difference between normal and operated animals is due to this? The data at hand do not permit of a certain answer to the question, but various lines of evidence point to the conclusion that irrelevant factors at least did not change a real inferiority of the operated animals into an apparent superiority: that the normal and operated groups are most probably equal in learning ability.

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The time required for solving the problem in successive trials during training by the animals in the different groups is summarized in table 3. Some of the animals learned the problem quickly and training was discontinued with them soon afterward. If their records are dropped out at the point where they finished practice the end of the learning curve will represent, not the average for the group, but the average of only those animals which learned slowly. To avoid this the time for the perfect records of each animal that finished early is included in the computation of the average for each period of later training with the group.

Judged by the time alone, the normal animals are somewhat superior to the operated ones, yet the superiority is by no means significant until after the fiftieth trial. The normal animals were but slightly superior to groups 3 and 5 in the time required for the perfect records (last period of training) and never equaled the record of group 4. Time records alone are probably never an accurate measure of the rate of learning and in this case they are apt to be particularly misleading since some of the paretic animals moved slowly throughout the experiments and, even when they made fewer errors, consumed more time than the normals.

The time records, then, give no accurate measure of the relative abilities of the groups. They do show roughly, however, that there is no very significant difference in rate of learning between the normal and operated animals.

The use of the problem box permits of a certain amount of variation in the standard of perfection required. Errors were recorded when the rat deviated from his usual path, but a good bit of latitude was allowed in the choice of that path. He might go in front of, across the top of, or behind the problem box in passing from platform *a* to *b*. So long as a stereotyped path was followed, learning was considered perfect. This allows some chance for the personal equation of the experimenter to modify the results. A partial check upon this source of error is provided by experiments upon the rate of formation of a habit of visual discrimination in which a definite invariable standard of error could be adopted. A description of this experiment follows.

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The formation of a visual habit

Methods. Seven normal and four operated animals were trained in a habit of brightness discrimination and their rates of learning compared. The apparatus used was a simple Yerkes' discrimination box (previously used and figured by Dodson, '17) offering the choice of an illuminated and a dark alley, food being given with the light and no food with darkness. If the animal

TABLE 5

The average percentage of errors made by normal animals and by animals after injury to the frontal pole of the cerebrum during successive days' practice in forming a habit of visual discrimination. The averages are based on seven normal and three operated animals trained under uniform conditions with ten trials per day

PER CENT ERROR	
Normal	Operated
61.8	73.3
52.3	50.0
42.7	66.6
30.0	30.0
18.6	30.0
24.3	26.6
14.3	13.3
7.2	3.3
5.7	3.3
11.4	3.3
5.7	0.0
1.4	0.0
5.7	
0.0	
0.0	

entered the darkened alley an error was recorded. Training was continued with 10 trials per day until 20 successive trials were made without error. Records of the animals follow.

Normal animals. The seven normal animals required an average of 106.6 trials for learning, not including the final 20 trials without error, with a range from 60 to 130. Details of their records will be given in another paper. Their daily averages are given in table 5.

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Operated animals. Four animals were trained after destruction of the frontal pole of the cerebrum. Three of them finished training and only limitation of time prevented the fourth from learning the problem. Their individual records are given below.

Number 1. Small female, 94 days old. The frontal pole of the cerebrum was injured by a transverse incision carried diagonally forward from the fronto-parietal suture. Training was begun 72 days after operation. She became ill during the latter part of training and had to be given a week's rest with full allowance of food.

Trials required for learning..... 100

Lesion: Plate III, figure 1. The frontal poles of both hemispheres, between and in front of the forceps of the callosum were completely destroyed.

Number 2. Small female, 94 days old. The frontal pole of the cerebrum was injured as in the case of number 1. Training was begun 72 days after operation.

Number of trials required for learning..... 70

Lesion: Plate III, figure 2. Practically all of both hemispheres in front of the vertical plane of the knee of the corpus callosum was destroyed. The left olfactory tract was severed. Both corpora striata were injured, that on the left most extensively.

Number 3. Small female, 73 days old. Operation as in the case of number 1. Training was begun 72 days after operation.

Number of trials required for learning..... 70

Lesion: Plate III, figure 3. All of the cortex lying above and between the forceps of the callosum was completely destroyed. The subcortical ganglia were uninjured.

Number 4. Medium sized male, 150 days old. Both frontal lobes were destroyed by transverse incisions. Recovery was very slow. The animal lay for four days in coma and appeared to be stuporous up to the beginning of training, 60 days after operation. During training he was very erratic, trying to climb out of the training box, or lying quiet for long periods. After 30 trials, however, he gave certain evidence of discrimination, turning back repeatedly before he reached the end of the dark alley, when errors were made, and advancing into

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the illuminated alley with a rush. Training was continued for only 50 trials, which did not permit of perfecting the habit but the secondary evidence of discrimination is conclusive.

Lesion: Plate III, figure 4. The frontal poles of both hemispheres in front of and above the knee of the corpus callosum were completely severed from the remainder of the brain, by an incision which passed through the olfactory tracts.

The total extent of the lesion in these animals is shown in figure 10. The average percentage of errors made on successive days by the normal and operated animals in these experiments is given in table 5. There is no very marked difference in the amount of improvement shown by the two groups from day to day.

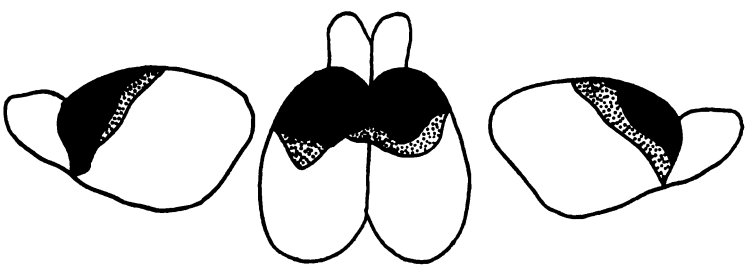

FIG. 10. THE TOTAL EXTENT OF THE LESIONS IN ANIMALS WHICH FORMED THE HABIT OF VISUAL DISCRIMINATION AFTER OPERATION

The stippled area represents the animal with which training was not completed.

The average number of trials required by the normal animals for learning the visual habit was 106.6 with a range from 60 to 130. The average number required by the operated animals was 80, with a range from 70 to 100. The operated animals thus show a superiority of 26.6 trials, or 24.9 per cent, over the normal animals. As in the experiments with the double-platform box, the operated animals are superior in learning ability to the normal controls.

In this case, however, the explanation offered before for the superiority of the operated animals does not hold good. The discrimination habit does not seem to be influenced by the relative activity, or vigor, of the animals and the use of a constant

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criterion of error effectively eliminates the personal equation of the experimenter. Are we then to conclude that the removal of a portion of the cerebrum does improve ability to learn? Such a conclusion seems preposterous, but there is no evidence at present from which I can deduce the flaw in the experiments. It is most probable that the difference between the groups is merely a chance result which would not hold if a larger number of cases were studied.

From the test it seems clear, at least, that the operated animals are able to learn as readily as do normal ones. The experiments have failed to reveal any retardation of the learning process resulting from the removal of the anterior third of the cerebrum.

The effects of destruction of deep-lying structures

The more important cerebral structures which lie below the surface and which might play some part in learning are the corpora striata, the hippocampal lobes, and the corpus callosum. Many of the animals trained on the double-platform box show extensive destruction of these structures and make it possible to test their function in learning.

The animals showing injuries to the hippocampal lobes were:

Number 5. Left hippocampal lobe injured.

Number 6. Left hippocampal lobe extensively injured.

Number 7. Right hippocampal lobe replaced by a cyst.

Number 8. Both hippocampal lobes slightly injured.

The average number of trials required for learning by these four animals was 81.2, which is practically the same as the average for all operated animals. The hippocampal lobes are not more important for learning than other parts of the cortex.

The animals showing injury to the corpora striata are:

Number 3. Complete destruction of the left C. S.

Number 4. Extensive injury to the right C. S. with probable interruption of its connections with the thalamus.

Number 5. Probable section of the connection of the left C. S. with the thalamus.

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Number 6. Complete destruction of the left C. S.

Number 13. Superficial injury to the right C. S.

Number 19. Severe injury to the left C. S., slight injury to the right.

The average number of trials required for learning by these animals was 53.0 which is slightly less than the average of paretic animals (most of them were paretic). The corpora striata are not of greater importance for learning than other parts of the cortex.

It has been claimed that injury to the corpus callosum results in some deterioration. Since the experiments have shown that the destruction of an entire hemisphere does not retard learning, the interruption of commissural fibers can hardly have a different effect. Actual test of this is provided by animals number 11, 14, and 18 which showed a very nearly complete destruction of the corpus callosum with cysts exposing the lateral ventricles. The average number of trials required for learning by these three animals was 83.6, which is not significantly greater than the average of all.

These records show that the structures mentioned have no special function in learning. In the various tests practically every deep-lying structure above the thalamus was injured and it seems clear that none of the cerebral nuclei is of any greater importance for learning than is the pallium.

IV. THE RELATION OF THE RATE OF LEARNING TO THE AMOUNT OF CEREBRAL INJURY

The apparent superiority of the operated animals in the foregoing experiment raises again the question of the real significance of the data obtained with the double-platform box and with the inclined-plane box in earlier work. Further evidence upon the point is furnished by a study of the relation of the rate of learning to the amount of cerebral material removed. This relation is of considerable interest from other standpoints also. If, as seems to be the case, there is no specialization of cerebral function for the acquirement of habits, learning may be the result either of

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the mass action of the cerebrum, as held by Loeb ('00) or of the formation of new functional reflex connections through any part of the cortex which happens to be intact. If the first assumption is correct, destruction of large areas of the cerebrum should retard learning more than lesser destructions.

The problem has been raised in a somewhat different way by the apparent reduction in learning ability of human defectives who show a deficiency in the cells of the pyramidal layer (Bolton, '05). The present evidence bears only indirectly upon this condition but it does seem that if deficiency in learning results from a reduction of the number of functional cells in one cortical layer it should also result from a reduction in the total number of cells, provided all are destroyed in like proportions.

To determine the percentage of the cortex destroyed by the various operations the total area of the cortex shown on the diagram used in plates I and II was measured. The area of the plotted lesions was then determined and, after correction for overlapping of the dorsal and lateral figures, was expressed as a percentage of the total area. This method gives only a rough measure of the extent of the lesion. As an absolute measure of the area destroyed it would be quite misleading, but since the same method was employed for all the data, it seems to give comparable figures for the different brains. The per cent of the total area of the cortex destroyed in each animal is given in table 6, ranked from the least to the greatest, together with the number of trials required for learning by each animal.

The average amount of destruction in all animals is 28.4 per cent. The average number of trials required for learning by all the animals with less than this amount of destruction is 88.1. The average number of trials required by those with more than the average destruction is 69.9. This gives a difference of 18.2 trials, or 20.6 per cent, in favor of the animals with the greater injury. But this analysis includes the animals with paresis (table 4). If we exclude them the averages become: for the group with less than average destruction, 82.8 trials, for the group with greater than average destruction, 93.2 trials, a difference of 11.1 per cent in favor of the group with lesser injury.

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This result is due, however, solely to the position of animal number 1, which showed average destruction and slow learning. If he is included with the lower group the relations are reversed to 10 per cent in favor of the group with greater destruction.

We may conclude, therefore, that the differences here are not significant. Paresis gives the animal a slight advantage in learning the double-platform box, but aside from this the extent of injury does not affect the rate of learning. Amounts of injury from 14 to 50 per cent of the entire cerebral cortex do not result in any correlated alteration in the animals' learning ability.

TABLE 6

The relation of the quantity of cerebral cortex destroyed to the rate of learning in animals trained on the double-platform box. On the left are the animals with less than the average destruction, on the right those with more than the average

PER CENT OF CORTEX DESTROYED	TRIALS FOR LEARNING	NUMBER OF ANIMAL	PER CENT OF CORTEX DESTROYED	TRIALS FOR LEARNING	NUMBER OF ANIMAL
14.2	78	15	29.3	99	14
15.5	90	16	30.4	49	6
15.2	81	12	31.5	41	13
(15.5)	Failed	(17)	31.9	27	19
17.8	97	8	32.1	107	7
20.3	82	10	40.7	132	2
21.2	101	11	40.8	45	9
24.9	51	18	42.4	75	4
26.6	72	5	50.0	54	3
27.0	141	1			
Averages 20.3	88.1		36.6	69.9	

None of this evidence really settles the question of the significance of the apparent superiority of the operated animals over normals. The data on paretic animals and on the relation of the extent of the injury to the rate of learning do suggest, however, that the reduction in vigor is the chief cause of the difference and also give some indication of the actual extent to which such reduction has influenced the results. The existence of an observable paretic condition is able to produce a superiority of 28 per cent over the learning rate of non-paretic operated animals (page 92). The difference in vigor between the non-paretic

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operated animals and normals is much less than between paretic and non-paretic and it is therefore very unlikely that it could have resulted in an acceleration of learning of more than 39 per cent, which is the difference found between non-paretic operated and normal animals. It can not, therefore, have altered a significantly reduced ability to learn into an apparent heightened ability. At most, it can only have obscured an actual equality. The evidence from the time consumed in training also fails to reveal a marked inferiority of the operated animals and there is a seeming equality in learning ability between animals with great and with small lesions.

All the data then seem to point toward an equality of normal and partially decerebrate animals with respect to learning ability. Certainly there is no evidence to show that the injuries to the cerebrum resulted in any reduction of the power to learn.

The bearing of the experiments on cerebral function in learning

The interest of the experiments lies, after all, not so much in the comparative rates of learning of normal and partially decerebrate animals as in the fact that the animals can learn at all rapidly after such extensive brain injuries. And on this point the experiments are absolutely conclusive. They have covered every portion of the cerebrum in one or another animal, and with bilateral operation every region except a small area at the base of the temporal lobes (figure 9). No single part of the cerebrum has proved to be necessary for the learning of the double-platform box and no single part has proved to be significantly more efficient in learning than any other part. For the acquirement of the habit the various parts of the cerebrum seem to be absolutely equipotential. Moreover, at least 50 per cent of the cortex can be dispensed with without marked deterioration and there seems to be no relation between the absolute quantity of cerebral cortex functional and the ability to learn.

Whether greater injury than 50 per cent would cause deterioration has not been determined. I have made some attempts to produce greater lesions but have not yet developed a satisfactory

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technique. In previous work Dr. Franz and the writer obtained formation of simple habits after somewhat more extensive injuries but unfortunately these animals were not tested in a more difficult habit.

What is the bearing of these results upon the problem of the cerebral mechanism of learning? In the first place, they show conclusively that there are in the rat no special association areas to which is restricted the function of associating sensory and motor projection areas. We know from earlier work that habits of the type required by the double-platform box are normally mediated by the frontal pole. But in the absence of this some other part assumes its function. The same condition must be true for any other so-called association area that may exist in the rat's cortex. There may be association areas which can function alternatively, but the operations have covered almost all combinations of cerebral regions except bilateral frontal with occipital and this possibility seems almost ruled out.

Second, the work of Franz on cats and monkeys and of Franz and the writer on the rat indicates that the cerebral reflex paths functioning in certain habits are distributed uniformly to all parts of the frontal region, since the destruction of one frontal lobe in the higher forms or of any given part of the frontal pole in the rat is not followed by loss of the habit and the destruction of the entire frontal region does result in loss of the habit. The present experiments indicate, further, that the organization of the entire cerebrum of the rat must be along similar lines; that the reflex connections involved in habits may be laid down in any part of the cortex. Under normal conditions the frontal pole perhaps offers less resistance to the reintegrating of the activities required by the habit than do other parts, but the same integrations may be formed readily elsewhere.

The determination of the regions which actually did function in the habit would have been of considerable interest. Franz found that when the animal lacking the frontal lobe learned a problem box, destruction of tissue adjacent to the first injury resulted in loss of the habit. On the other hand, Leyton and Sherrington ('17) failed to locate the region which assumed motor

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control of the thumb in the chimpanzee after the normal area had been destroyed. They did determine that the function was not taken up by adjacent areas. It would have been possible to make such determinations in the present series by operating subsequent to training but a second operation makes it difficult to delimit the first lesion exactly and it seemed best to restrict the present work to the investigation of the vicarious functioning of the cerebrum and to postpone the working out of the exact areas functioning to a later time.

Third, the existence of a special area for attention or for "higher psychic functions" is ruled out. If there is any difference, the operated animals show somewhat more definite limitation of behavior to reactions to the problem situation than do normals and the double-platform box certainly demands "intelligence" if we can use the term in reference to any animal.

Finally, the experiments raise another question of fundamental importance. What becomes of the concept of sensory and motor projection areas if the entire cerebral cortex of the rat is equipotential in learning? The following sections of this paper record an attempt to answer this question.

V. THE FUNCTION OF THE CEREBRUM IN THE RETENTION OF A HABIT OF VISUAL DISCRIMINATION

The formation of the double-platform box habit involves at least the linking up of tactile and kinaesthetic impulses with motor reactions. Other sense organs probably play only a minor rôle. We do not know the position of the kinaesthetic projection area in the rat, but it probably has some such position as in the cat (Barenne, '16) where it seems to be not clearly separated from the motor area. Whatever its position, if it is definitely localized, it was certainly destroyed in some of the experiments, yet none of the animals gave evidence of loss of kinaesthetic impulses. The exact function of these in the behavior of the rat is difficult to determine, however, and it seemed desirable to have data on learning involving some stimulus which can be experimentally controlled, in order to determine whether there are

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definite areas corresponding to the sensory projection areas of higher forms and what the relation of these to habit-formation may be.

Such data will be of value also for the elucidation of another problem. In his work on the frontal lobes Franz was inclined to take the view that they acted as an association area, that they were functional in learning because it was learning and not merely because they are the normal projection areas for some group of afferent or motor impulses. His experiments dealt only with various types of latch-boxes, the habits for all of which probably have a kinaesthetic foundation. If his view is correct, the destruction of the frontal lobes should result in the abolition of any complex, recently formed habit. If, on the contrary, the frontal lobes serve simply as a projection area, some sensory habits should be found which would survive their destruction and be eliminated by injury to other parts.

With these questions in mind I took up the study of a visual habit; discrimination between light and darkness.

Apparatus and methods

For setting up the habit a Yerkes' discrimination box of the customary form was used. (The particular box has been figured by Dodson, '17). The box offers a choice of two alleys and at the end of each alley a circular, translucent stimulus plate was fixed. In each trial one of these was illuminated, the other dark. A number of normal animals were trained to go toward the illuminated plate and to avoid the dark one. No attempt was made to restrict the stimulus to the plates and it is probable that the animals reacted to the general illumination of the alleys (Lashley, '12). Precautions were taken, however, to insure that the reaction was to the visual stimulus. I have discussed the control of visual stimuli extensively in other papers ('12 and '16) and need not review the controls here.

The animals were given 10 trials per day (food with the positive, no food with the negative stimuli) and training was continued until errorless records were obtained on two successive

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days, a total of 20 successive errorless trials. Entrance into the darkened alley was counted as an error.

Since the operation entails a period of from one to five days without practice it was necessary to determine the loss of the habit which this rest alone would produce. Therefore, when each animal had learned the problem he was kept for seven days without practice and was then tested for retention of the habit. These tests are recorded as "preliminary retention tests" in the records of experiments. In case a loss of habit appeared after this rest period the animal was retrained until he made 20 successive errorless trials. When these were completed he was subjected to operation.

For the experiments the animals were divided into four groups. The number of animals in each group and the character of the

TABLE 7

Distribution of animals in experiments on discrimination of brightness

GROUP	CHARACTER OF LESION	NUMBER OF ANIMALS
1	Frontal pole destroyed	2
2	Parietal regions destroyed	2
3	Skull opened in occipital region, no injury	1
4	Occipital regions destroyed	3

operation are shown in table 7. As soon as they had recovered from the shock of operation, in from 24 to 96 hours, they were tested in the discrimination box for evidence of retention. At least 30 trials were given in the postoperative retention tests. Two sorts of evidence are important in interpreting the results. First, the postoperative retention tests may show no more error than the preliminary retention tests. They then give conclusive evidence of retention. Second, when the postoperative retention tests show numerous errors the animal may yet give evidence of retention by selective reactions to the stimuli.

At one stage in the formation of a visual discrimination habit the rat begins to give evidence of attention to the lights. The first trials are evidently random dashes and in case of a wrong choice the rat does not turn back until he has actually been pre-

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vented from reaching the food. At a later stage he hesitates before choosing an alley, advances toward the negative stimulus hesitatingly and turns back before he reaches it. Later he may stand at the entrance to the two alleys and sway back and forth between them, finally choosing the positive stimulus. Such behavior is easily recognizable and is the invariable precursor of accurate discrimination. In the following records it is referred to as "comparison-movements."

After the postoperative retention tests the animals were killed and their brains examined in serial sections. The records of the separate tests follow.

Group 1. Frontal pole of cerebrum destroyed.

Number 5. Small male, 109 days old. Trained on brightness discrimination.

Number of trials required for learning..... 100
Preliminary retention tests, per cent error..... 4

The frontal pole of the cerebrum was injured by transverse incision. On the following day the animal was very weak, scarcely able to reach the food, but his reactions were unhesitating, with comparison movements. Only one error was made in ten trials. On the second day he was stuporous and was not tested. On the third day his reactions were quick and discrimination was unmistakable.

Postoperative retention tests, per cent error..... 10

Lesion: Plate III, figure 5. Both frontal poles were completely destroyed by a section passing diagonally forward from the fronto-parietal suture along the forceps of the callosum to the base of the olfactory bulbs.

Number 6. Small male (castrated), 108 days old. Trained in brightness discrimination.

Number of trials required for learning..... 50
Preliminary retention tests, per cent error..... 5

Operation was as in the case of number 5. On the day following operation his reactions were quick. One error was made in ten trials with comparison-movements and negative reactions to darkness. Later trials verified the first day's record.

Postoperative retention tests, per cent error..... 10

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Lesion: Plate III, figure 6. The dorsal convexity of both frontal poles was completely destroyed.

Both these animals gave certain evidence of discrimination within 24 hours after the operation. In both the frontal pole was completely destroyed. They had not been overtrained, so that there is no reason to believe that a once cortical habit had been reduced by practice to lower levels. It is probable that the frontal pole of the cerebrum is not functional either in the formation or the retention of the habit of visual discrimination.

Group 2. Parietal regions destroyed.

Number 7. Small male, 130 days old. Trained in brightness discrimination. Was always erratic and given to exploration.

Number of trials required for learning	220
Preliminary retention tests, per cent error	7.5

The parietal areas of both hemispheres were injured by incision through two trephine holes extending backward from the fronto-parietal suture. Retention was tested on the following day. He was then excited and restless, his reactions were quick, but showed an exploratory character, an exaggeration of his previous behavior. On the second day he was stuporous and was not tested. On the third and later days he made no error and gave secondary evidence of perfect discrimination.

Postoperative retention tests, per cent error	0
(*After first day*)	

Lesion: Plate III, figure 7. The central portion of the convexity of both hemispheres was replaced by cysts. The degenerated cortex extended from the knee of the corpus callosum caudad to the anterior margin of the hippocampus, laterad from the longitudinal sinus to the orbital surfaces.

Number 8. Small male (castrated), 115 days old. Trained in brightness discrimination.

Number of trials required for learning	60
Preliminary retention tests, per cent error	2.5

Operation was as in the case of number 7. Retention was tested on the day following operation. For several days after operation the rat's

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right eye was closed. On the first day he seemed to discriminate correctly whenever, in starting, his left eye was directed toward the illuminated alley. There was a marked tendency to circus movements towards the functional eye. On the second day he was not tested. On the third and later days he made no errors and there was unmistakable secondary evidence of discrimination.

Postoperative retention tests, per cent error	20
After first day	0

Lesion: Plate III, figure 8. The lesion is almost identical with that of number 7, but more complete in the mesial region.

After destruction of the greater part of the parietal and orbital regions these animals gave good evidence of the retention of the discrimination habit. If we disregard the records of the first day (24 hours after operation) when both animals were in an abnormal condition, we find that they showed perfect retention. Like the frontal pole, the parietal and orbital regions are not functional in the visual habit.

Group 3. Occipital control. To determine whether or not the shock of operation in the occipital region would produce loss of the habit, irrespective of injury to the brain, I trephined the skull in one animal in the region through which the occipital operations were to be performed.

Number 9. Small male (castrated), 115 days old. Trained in brightness discrimination.

Number of trials required for learning	60
Preliminary retention tests, per cent error	2.5

Two openings were made in the skull about two millimeters in front of the parieto-occipital suture, similar to those through which operations on the occipital lobes were performed in other animals. The brain was left with no other injury than that occasioned by opening the skull. Retention was tested on the following day. Reactions were prompt and without error. His behavior was quite uninfluenced by the operation.

Postoperative retention tests, per cent error	0

Lesions: Plate III, figure 9. Except for adhesions in the area of the trephine holes the brain was uninjured.

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This animal showed perfect retention after operation. Opening the skull in the occipital region does not produce sufficient shock to cause loss of the habit.

Group 4. Occipital region destroyed.

Number 10. Small male, 114 days old. Trained in brightness discrimination.

Number of trials required for learning	60
Preliminary retention tests, per cent error	2.5

The occipital lobes were injured by passing a knife backward from openings in the skull, like those in number 9, to the base of the skull, avoiding injury to the thalamus and cerebellum. Retention was tested on the following day. The animal was active and gave little evidence of discomfort. He went through the problem box promptly and without hesitation. He found the food readily and corrected his errors promptly, but he made six errors in ten trials. On the following day his behavior was like that of an animal in the early stages of training. He retained all of the habits of the discrimination box except that of visual discrimination. Retraining was begun and for four days he gave no evidence of discrimination. He then improved until the reaction was reestablished.

Postoperative retention tests, per cent error	50
Trials required for relearning	100
With errors distributed on successive days as follows	6, 5, 5, 3, 3, 0, 1, 0, 2, 2, 0, 0

Lesion: Plate III, figure 10. On the left the lesion included all of the cortex caudad to the plane of the posterior limit of the hippocampus. On the right the mesial surface of the occipital lobe remained intact.

Number 11. Small male, 107 days old. Trained in brightness discrimination.

Number of trials required for learning	60
Preliminary retention tests, per cent error	2.5

The occipital lobes were injured by an operation similar to that on number 10. Retention was tested on the day following operation. The animal was very active, extremely hungry, and when he reached the food ate voraciously. He seemed unable to adjust himself to making the turn into the food compartment and after the fourth trial

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persisted in climbing out of the discrimination box. On the second day he responded promptly and found the food readily, but developed a position habit which persisted for three more days. Training was continued until he relearned the problem.

Postoperative retention tests, per cent error..... 65
Number of trials required for relearning..... 74
With errors distributed on successive days as follows
3 (of 4), 6, 4, 9, 2, 0, 3, 4, 0, 0

Lesion: Plate III, figure 11. On the right hemisphere the lesion passed through the dorsal surface of the hippocampus and included all the cortex laterad and caudad to this structure. On the left hemisphere the lesion was less extensive and included only the cortex dorsad and caudad to the hippocampus.

Number 12. Small male, 115 days old. Trained in brightness discrimination.

Number of trials required for learning..... 60
Preliminary retention tests, per cent error..... 0.5

The occipital regions were destroyed as in other cases. Retention was tested on the following day. He found the food readily and corrected his errors promptly, averaging less than two seconds per trial. During 60 trials on six successive days he gave no evidence of visual discrimination. Retraining was not undertaken.

Postoperative retention tests, per cent error..... 55

Lesion: Plate III, figure 12. The dorsal convexity of both occipital poles was destroyed.

These three animals after extensive destruction in the occipital region showed complete loss of the habit of visual discrimination. Evidently the visual habit is mediated in some way by the occipital region and by the occipital region alone. Two of the three animals relearned the habit in normal time; the third was not tested adequately. The occipital lobes, though normally functional, are not necessary for the formation of the visual habit.

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Summary and discussion

The total extent of the injuries in the frontal and parietal regions (animals, 5, 6, 7, and 8) is shown in figure 11. Practically all of the anterior two-thirds of the cerebrum was covered by these tests. The animals all gave unmistakable evidence of retention of the habit and the slight reduction in accuracy in some cases is no more than would be expected from the general systemic results of the operation.

The experiments reported on page 94 ff. show that the frontal regions of the cerebrum are not necessary for visual discrimination. These experiments show, in addition, that the perfected habit of visual discrimination is not mediated by the frontal,

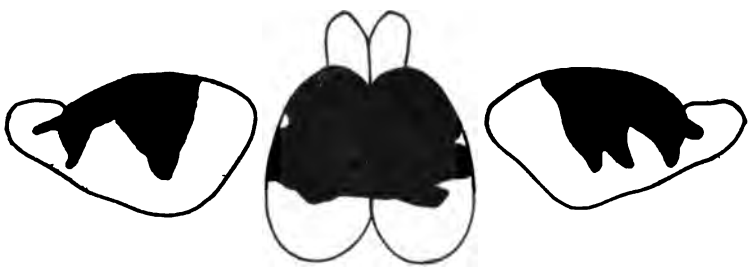

FIG. 11. THE TOTAL EXTENT OF LESIONS IN ANIMALS WHICH RETAINED THE HABIT OF VISUAL DISCRIMINATION AFTER OPERATION

parietal, or orbital surfaces of the cerebrum, and that these areas probably have no part in its normal formation.

The last three cases give equally clear evidence that the occipital pole is intimately concerned with habits of visual discrimination. Table 8 gives their retention records in comparison with the others. Every one of the animals lost the habit completely after injury to the occipital pole. That the loss was due to cerebral destruction and not to the mere shock of operation is indicated by the results with number 9, and by the fact that equally extensive operations in other regions adjacent to this produced no disturbance in behavior.

These data justify the conclusion that the entire cortical mechanism of the perfected visual habit is included within the

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occipital pole of the cortex. This is not at all in accord with current views concerning the mechanism of habit-formation. Von Bechterew ('11, p. 2020) has summarized the more commonly accepted view in these words:

> Since from earlier observations in my laboratory (Dr. Protopopov) it appeared clearly that in the production of the association reflex the motor reaction is mediated through the motor cortex and the perception of the associated stimulus occurs in the cortical perceptual or so-called sensory centers (at least for sound, light, and contact) whose localization is now well known, it follows that the excitation of the motor reactions is induced by association fibers which must connect these perceptual or sensory centers with the motor area.

TABLE 8

The percentage of errors made by animals tested for retention of the habit of brightness discrimination before and after cerebral operation

NUMBER	CHARACTER OF LESION	PER CENT ERROR	
		Preliminary retention tests	Postoperative retention tests
5	Frontal	4	10
6	Frontal	5	10
7	Parietal	7.5	0
8	Parietal	2.5	0
9	Occipital control	2.5	0
10	Occipital	2.5	50
11	Occipital	2.5	65
12	Occipital	0.5	55

This interpretation, derived largely from data on the dog and man, does not hold good for the rat. In it, at least in visual habit, long transcortical connections are not formed. The same area which functions as a sensory projection area seems to contain also corresponding motor fibers. A discussion of the bearing of this result upon the condition in higher animals will be left until one additional line of evidence upon the vicarious functioning of the cerebrum has been presented (section VI).

Is the loss of the habit an indication of a general "psychic blindness" or is it due only to the interruption of specific reflex arcs involved in the habit? As far as could be determined the

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animals could see perfectly well after the occipital destruction. Vision in the rat is difficult to detect without prolonged training, but some indication of vision is given by the following observations. The animals would jump up and grasp the edges of the training box accurately although the depth of the box was so great that they could not reach the top of it with their vibrissae, even when they stood on tip toe. They would also leap across a space too broad to be spanned by their vibrissae but showed greater hesitation in this than do normal animals. Finally they relearned the problem in no greater time than the average of normal animals, which could scarcely have occurred had they been handicapped by cortical blindness. The operation which destroyed the visual habit did not destroy the ability to see.

The destruction of the occipital pole did not result in a loss of all habits, but only in the loss of the visual one. The accurate running of the discrimination box requires not only the formation of the sensory habit but also a great many adjustments to the box. The animal learns to go directly through the alleys to the food, to turn back and pass through the discrimination compartment and second alley without a pause in case the wrong alley was chosen first, to claw at the door of the starting compartment in case the experimenter is slow in releasing him, and to grab a double handfull of food as the experimenter transfers him to the starting compartment. All these activities appeared in the rats with occipital lesions. The tactile and kinaesthetic motor habits were retained, only the visual habit was lost. Von Monakow ('14) has criticized Franz's experiments on the ground that they merely produced a 'protracted clouding of the sensorium' similar to that following skull-fracture in man and that the loss of habitual reactions after injury to the frontal lobes was merely a diaschesis effect (shockartige Störung). The complete elimination of one habit with the perfect retention of other probably no less complicated ones after occipital lesions speaks very strongly against Von Monakow's view and in favor of the one advanced above, that the loss is due to the interruption of specific reflex arcs.

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The exact delimitation of the visual area has not been made possible by these experiments, but the gross area determined seems to correspond to the histological findings. The majority of writers have localized the visual cortex of rodents on the posteromedial aspect of the cortex, behind the caudal fibers of the corpus callosum and in contact with the cerebellum (Isenschmid, '11). This region remained uninjured in all but one of my cases. It probably is not the part functional in the visual habit. Brodmann ('09), on the other hand, locates the area striata on the dorsal convexity of the occipital pole in the rabbit and Isenschmid

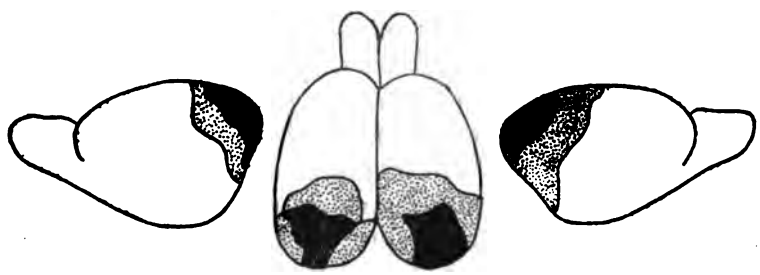

FIG. 12. THE TOTAL EXTENT OF THE LESIONS IN ANIMALS WHICH LOST THE HABIT OF VISUAL DISCRIMINATION AFTER OPERATION

The stippling shows the total area, the solid black the area common to all.

('11) is inclined to favor this view in his studies on the cytoarchitecture of the mouse's brain. Figure 12 shows the combined extent of the lesions in the three occipital cases. Only a small area on the dorsal convexity was destroyed in all three animals. This covers approximately the region described by Brodmann for rodents as the area striata. It is by no means certain, of course, that the region destroyed was the region functional in the habit, but the mass of evidence on cerebral function accumulated from the rat gives little indication of shock effects and the most probable interpretation of the data is that the loss of habit resulted from the destruction of Brodmann's area striata.

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VI. THE STIMULABLE CORTEX AND THE CORPUS STRIATUM IN RELATION TO DISTURBANCES OF MOTOR COÖRDINATION

The investigations of the functional anatomy of the rat's brain thus far reported have all pointed to an almost complete interchangeability of function among the different parts of the cortex, and a complete and rapid recovery from the effects of cerebral operation. But among the 60 odd rats which have been subjected to operation and subsequent tests by Dr. Franz and the writer, there have appeared 9 with a persistent hemiparesis. Ordinarily, paralytic symptoms clear up very quickly, if they appear at all, in lower animals after cerebral destruction. In these animals, however, the paretic condition, affecting the fore and hind limbs of one side with sometimes the face, seemed to be almost permanent. Some of the paretic animals were kept under observation for four months and during this time made no perceptible spontaneous improvement in motor coördination. This persistent paresis is so strongly in contrast with the evanescent loss of other functions after cerebral injury as to arouse a special interest in its cause. Franz and the writer suggested a possible relation of the paresis to lesions of the corpus striatum but the material then available was not varied enough to settle the point. The present experiments have produced six additional cases and these give fairly conclusive evidence on the cause of the paresis.

The hemiparesis is easily recognizable by inspection as a stiffness, clumsiness, or weakness of the legs of one side. It usually leads to marked disturbance of gait, such as rotation to the right or left, but the direction of rotation has no constant relation to the position of the paralysis. A somewhat more certain analysis of the character of the motor disturbance was obtained by forcing the animals to climb down a vertical stretch of wire netting. Normal animals support their weight equally well with the feet of either side. Paretic animals can grasp firmly only with the feet of the sound side and fall if forced to turn so that the weight must be supported by the paretic legs. This test was applied only to animals in the later experiments and the data in these are therefore the more reliable.

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The electro-stimulable cortex of the rat

The portion of the rat's cortex from which movements may be elicited by electrical stimulation have been described only by Ferrier ('76) and only in the barest outline. A more exact determination of its extent is necessary for this comparison. I have mapped the excitable cortex in about 25 animals. There is a great deal of variation an account of which must be postponed until a comparison of anesthetics is possible. Figure 13 is a composite picture of the excitable areas of these animals.

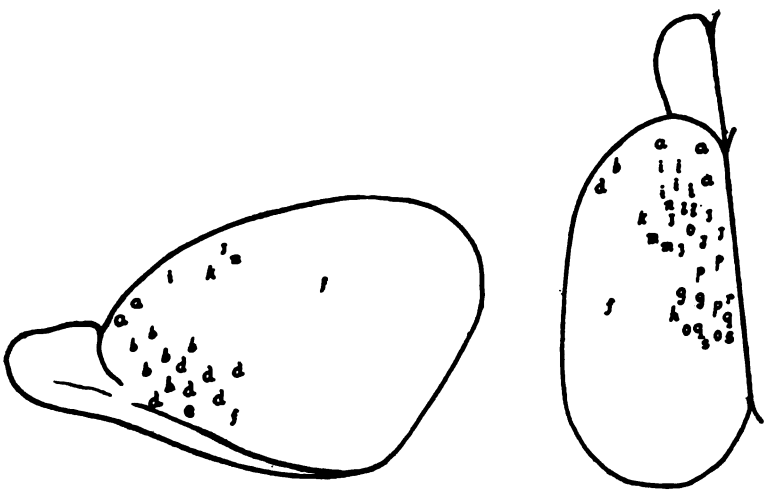

FIG. 13. DIAGRAM OF THE EXCITABLE CORTEX OF THE RAT

For the significance of the letters see text.

a. Head turned to opposite side
b. Nose retracted
c. Vibrissae moved
d. Chewing movements
e. Tongue protruded
f. Eye closed.
g. Ear laid back
h. Ear pricked up
i. Shoulder drawn forward
j. Fore-arm retracted
k. Elbow flexed
l. Elbow extended
m. Wrist flexed
n. Fore-arm rotated
o. Back flexed to opposite side
p. Hind leg drawn forward
q. Homolateral leg flexed, ipselateral extended
r. Ankle extended
s. Tail drawn to opposite side.

All the movements elicited from all animals are shown, so that the diagram includes many more movements than can be ob-

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tained from any one animal. These movements, with their designation in the figure are listed above. Unless otherwise indicated the movement is on the contralateral side.

The stimulable area embraces all of the frontal pole in front of the forceps of the callosum and the dorso-medial portion as far back as the anterior margin of the hippocampal lobes. The responses elicited correspond roughly to the antero-posterior distribution of the muscles involved, the head having the most anterior representation. The responses are obtainable only under light anesthesia and are less well coördinated than in higher forms.

The relation of the stimulable cortex to paresis

In primates destruction of the stimulable cortex produces nearly complete paralysis, in the dog the paralysis persists for only a few days, in the rat destruction of this area produces no discoverable disturbance. The data included in this and the earlier studies contain a large number of cases of bilateral destruction of the motor cortex. Animals 4, 5, and 8, section III, of this paper all show extensive destruction of the stimulable areas of both hemispheres. Numbers 1, 3, 4, 20, and 28 of the series of Lashley and Franz ('17) show similar superficial bilateral lesions. None of these animals showed any persistent motor disturbance, although in many the destruction of the stimulable areas, either of one or both hemispheres, was practically complete. The paresis described does not result from destruction of the motor cortex.

The character of the lesions in paretic animals

The animals showing paresis and the character of the lesion in each are shown in table 9. Animals from the earlier series of Lashley and Franz are included with those of the present series. Since the experiment numbers of the two series are the same the animals of the former series are indicated by an asterisk. For detailed descriptions of the behavior and extent of the lesions in those animals the reader is referred to the original paper.

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The lesions of the animals of the present series are described in section III.

Of the cases included in the table one (number 14*) must be excluded because the brain was not sectioned, leaving eight significant cases. Two lesions are common to seven of these eight. They are destruction of the motor cortex and destruction of *one* corpus striatum. Number 14 alone shows no injury to the corpus

TABLE 9

The character of lesions in animals showing paretic symptoms

NUMBER	PARESIS	DESTRUCTION OF MOTOR AREA	DESTRUCTION OF CORPORA STRIATA	OTHER LESIONS
14*	Left	Brain lost		
38*	Left	Complete	Right degenerated	Right fornix
39*	Right	Complete	Left degenerated; Right injured(?)[1]	Extensive
3	Right	Unilateral	Left degenerated	Left hemisphere complete
4	Left	Unilateral; nearly all	Right posterior part destroyed	Extensive
5	Right	Unilateral; nearly all	Left severed from thalamus	Extensive
6	Right, cleared up	Anterior part intact	Left destroyed	Fornix
14	Right	Fore arm and leg regions	Uninjured	None
19	Right	Bilateral	Left degenerated	Slight

[1] In the earlier description of this animal it was stated that the lesion passed through the right corpus striatum. In reëxamining the sections I find that the cut passed along the external capsule laterad to the C. S., and produced at most a very slight injury to it.

striatum and his paresis is questionable (p. 120). These are the only lesions common to all the paretic animals. Injury to the motor cortex alone has been shown to be incapable of producing paresis. There remains only the injury to the corpus striatum as a possible explanation of the motor disturbance. A further fact points to the corpus striatum as the source of paresis. In every case the paresis was on the side opposite the injured nucleus, irrespective of whether the lesion to the stimulable cor-

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tex was bilateral, as in numbers 38*, 39*, 14, and 19, or unilateral, as in the others.

But the immediate reference of the paresis to the corpus striatum was prevented by the fact that many of the animals which gave no indication of motor disturbance showed more or less extensive injuries to the corpora striata. This made necessary the reëxamination of the entire mass of material to determine the exact character of the lesions to the corpora striata in each case. The results of this are summarized in table 10. The lesions were worked up without reference to the behavior of the animals and classified as bilateral, unilateral, slight, and extensive. They were then coördinated with the behavior material. The result is a striking correspondence between extensive lesion to one corpus striatum and the appearance of the paresis. The table shows that no animal with bilateral injuries developed paresis: that no animal with slight injury to one corpus striatum developed paresis: that all but one animal (number 36*) with extensive destruction of one striate body showed pronounced hemiparesis. Horizontal sections from the brains of typical cases in each of these three groups are shown in plate IV, in which the figures bear the experiment numbers of the series of Lashley and Franz. Similar sections of all of the animals of the present series are given in plates I and II. From these a good idea of the actual extent of the injuries can be obtained.

Of the animals with extensive lesions to one striate nucleus only number 36* failed to show pronounced paresis. A review of the original description of this animal revealed the fact that the animal had an initial preference for the right turn in the simple maze. A similar tendency to turn to one side occurs in all paretic animals and is much less frequent in normal animals. Further the direction of turning is that which most frequently results from right paresis (lesion in left hemisphere) so that it appears that this animal had a slight paresis which had remained undetected. If this interpretation is correct, it seems that every animal of the series which had an extensive destruction of *one* corpus striatum developed hemiparesis, and that this lesion is the only one of those recorded which does produce motor disturbance.

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One apparent exception to this rule still remains. It is number 14 (table 9) reported as having paresis. Reëxamination of the original data on this animal shows that the diagnosis of

TABLE 10

Summary of the condition of the corpora striata in all animals showing injuries to these structures, with the state of motor coordination. Sections of brains from the earlier series (*) are figured (f) in plate IV; and from the present series in plates I and II

NUMBER	LESIONS TO C. S.	MOTOR CONDITION
	Both corpora striata injured	
10*f	Both cut, not degenerated	Normal
11*f	Both injured and compressed by enlarged ventricals	Normal
25*f	Both extensively injured	Normal
26*f	Both transected in anterior region	Normal
27*f	Both extensively injured	Spastic
29*f	Both cut transversely	Normal
30*	Both cut transversely in anterior region	Normal
32*	Both injured extensively	Spastic
34*	Both much shrunken (?)	Spastic
	One corpus striatum slightly injured	
22*f	Left transected	Normal
23*f	Left transected in anterior region	Normal
33*	Left, anterior third	Normal
37*f	Right very slightly injured	Normal
	One corpus striatum injured extensively	
36*f	Left destroyed	Paresis (?)
38*f	Right degenerated	Paresis
39*f	Left destroyed	Paresis
3f	Left destroyed	Paresis
4f	Right severed from thalamus	Paresis
5f	Left severed from thalamus	Paresis
6f	Left destroyed	Paresis
19f	Left destroyed	Paresis

f, Figured; * first series.

paresis was made solely upon the fact that he carried his head twisted to one side. He supported his weight with the feet of either side. The twisted position of the head frequently devel-

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ops spontaneously and is probably due to cerebellar or semicircular canal trouble. It is possible that the condition in this case was of a similar sort and not due at all to the cerebral lesion.

In all the certain paretic cases described there was extensive lesion to the stimulable cortex as well as to the corpus striatum. Lesions to the stimulable cortex alone do not produce motor disturbance. Does lesion in the corpus striatum produce a paretic condition when the stimulable cortex is intact or do these two structures have an interchangeable function as Luciani ('15) suggests? In one case only, number 6 (plate I, figure 6), was any large portion of the stimulable cortex intact. The descriptions of this animal made subsequent to operation record a slight right paresis. This was no longer detectable, however, when training was begun, 30 days after operation. One case can not prove the point, but it suggests that the motor cortex may be able to compensate completely for the loss of the corpus striatum.

Bilateral injuries to the corpora striata are not followed by paresis. The spasticity noted in some of the cases (table 10) may perhaps be ascribed to the bilateral lesion since it appeared in animals having very extensive lesions, but none of these animals was kept long enough to assure that the spasticity was not the result of the general cerebral condition following operation. Moreover, a similar spasticity appeared in animals without lesions in the corpora striata, so that the function of these nuclei in producing it is questionable. Except for the few cases of spasticity the animals with bilateral injuries to the striate nuclei showed no motor disturbances. They were able to climb actively and showed no such weakness as appeared in hemiparesis. In animals number 10*, 11*, 27*, and 29* the injuries to both striate nuclei were very extensive but there is no case in the series in which destruction of both nuclei was as great as that of one in the hemiparetic animals. It may be that the lack of motor disturbance in these animals is due only to the incomplete destruction of the nuclei, but in number 29* (plate IV, figure 29*) there is indication of a complete section of the anterior two-thirds of the right nucleus from the thalamus and in numbers 10*, 11*, 25*, and 27* also the lesions to one or both nuclei were very extensive.

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These cases suggest that there is an actual difference in the effects of unilateral and bilateral destruction; that destruction of the corpus striatum does not actually eliminate the coördinating mechanism, but merely destroys the balance of some regulatory mechanism which is useful when functioning normally but which may be dispensed with altogether provided that the loss is bilateral. Such a situation would be analogous to that presented by the semicircular canals, but more experimental data must be accumulated before the point can be established.

The function of the motor area in learning

These observations on paresis indicate that the stimulable cortex and some part or all of the corpus striatum have a similar motor function. Is it possible that neurones in either one of these structures may function as the final common path of a learned reaction in the absence of the other? If this were true it would explain the seeming restriction of visual habits to the occipital pole (section V) and the failure of complete destruction of the excitable cortex to abolish simple kinaesthetic-motor habits. The records include two cases which speak strongly against this, however. They are animals number 10* and 11* (plate IV, figures 10 and 11). Both of these animals showed complete destruction of the excitable cortex as a result of the two successive operations to which they were subjected and also very extensive injuries to the corpora striata. Nevertheless, both showed practically perfect retention of the simple maze. It is possible that the portions of the corpora striata remaining were sufficient to mediate the habit, but it seems unlikely that such extensive injuries to a functional area should not have produced some deterioration. Further, a number of cases (table 9) appear in which extensive injury to both corpora striata and the excitable cortex was not followed by any marked loss in ability to learn. Such results suggest that the motor areas may not be important for learning as such; that the function of the motor cortex may be maintenance of normal muscle tone and that habitual reactions may be superimposed upon this by impulses

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descending directly from other cortical regions. The problem is an interesting one and can be readily tested by careful operations.

VII. THE SIGNIFICANCE OF THE GENERAL RESULTS FOR THE CEREBRAL MECHANISM OF LEARNING

The experiments recorded in this paper give no indication of the character of the reintegration of conduction paths which occurs in learning. That problem must wait for a physiological analysis of the factors which determine the direction of conduction in the nervous system and for much more detailed studies of the behavior of small groups of neurones in learning. But the experiments do give some insight into the function of the anatomical divisions of the cerebrum in the acquirement and retention of habits and the course of the conditioned-reflex arcs through which learned reactions are mediated.

The point most clearly demonstrated is the complete vicarious functioning of all parts of the cerebrum in learning. Not only are areas to which associative function might be ascribed unnecessary for the acquirement of habits but even projection areas, which have been considered as hereditarily organized relay stations for impulses to or from the cortex, may be dispensed with without any discoverable loss in ability to learn a difficult reaction which is normally mediated by those areas. This capacity for vicarious functioning seems to hold true both for the cortex and for the underlying structures making up the archipallium. Such a fact can only be explained by the assumption of a fan-like distribution of fibers from the lower centers to all parts of the cerebrum, permitting incoming impulses to reach any part of the cortex, which happens to be intact, with equal ease.

Yet if this is the case, it is not clear why any particular part of the cortex should function to the exclusion of other parts in the formation of a habit under normal conditions, unless we can further assume a somewhat greater concentration of fibers from given lower centers in given cerebral areas. Probably this represents the true state of affairs, for it is evident from the fact that

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destruction of the frontal or occipital regions abolishes corresponding habits that under normal conditions the various parts of the cerebrum have specialized functions. Nevertheless this specialization is only relative and is of such little practical consequence that learning may go on with equal speed in the presence or absence of the specialized areas.

The results further form a strong argument against the existence of any cerebral areas which have a directive influence over learning, whether it be by "attention," mediated through the frontal lobes or by the "conscious action" of the brain as a whole. On the contrary, they suggest that the only essential condition for learning is the simultaneous activity of two reaction systems which are in anatomical connection by association fibers.

By far the most suggestive of the results brought out by the experiments are those dealing with the function of the occipital pole and the corpus striatum. It is clear that the habit involving discrimination of brightness has no other cortical representation than the occipital region. The conditioned visual reflexes do not pass across the cortex to association or motor areas. The corpora striata seem to have an alternative function with the stimulable cortex for the control of motor coördination. Two possible hypotheses concerning the path of conditioned-visual-reflex arcs are thus admitted. Either the impulses pass to the visual projection area in the occipital pole and thence to the corpus striatum, or they pass to the visual area and directly back to lower centers without traversing any long association fibers or reaching a cerebral motor area. Some evidence was found that the corpora striata are not necessary for the acquirement of motor habits and that the habit of the simple maze may be retained after the destruction of both the stimulable cortex and the corpora striata (section VI). The data on this point are not conclusive, but seem sufficient to raise the question of whether or not the neurones of the motor areas of the cerebrum really serve as the final common path of learned reactions. The possibility does not seem excluded by any evidence that I know of that the loss of motor control in paralysis is due, not to the interruption of conditioned-reflex paths, but to some disturbance in tonic

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innervation. The loss of voluntary movement in hemiplegia, pure motor aphasia, etc., might thus be due solely to the disturbance of some fundamental coördinating mechanisms, upon which the habitual reactions are superimposed, rather than to interruption of the habit-mechanisms themselves.

Existing evidence of the motor function of the corpora striata has been rather uncertain. Luciani ('15) and others have obtained movements from stimulation of the caudate nucleus but Von Bechterew ('09) claims that this was due to spread of current and that no movements can be elicited if degeneration of the internal capsule is first induced. The data given in section VI seem to show the motor function of the striate nucleus conclusively.

With what justice may results on cerebral function found for the rat be extended to higher forms? The rat has a very primitive cerebral organization but I doubt that this justifies the assumption that there is any fundamental difference in cerebral mechanism between the rat and even man. The chief arguments which have been advanced for the greater specialization of cerebral function in higher forms have been the lack of paralysis from destruction of the motor cortex in animals below the primates, rapid recovery from sensory defects after destruction of afferent projection areas in lower forms, and the increasing number and complexity of histologically distinct cortical areas with advancement in the evolutionary scale. The significance of the last point is questionable. The rat has many areas distinct in cytoarchitecture but these seem to have no absolute functional significance. The data on the striate nuclei suggest that the motor area is quite distinct in the rat, but with a predominance of function in the subcortical nuclei which is assumed by the motor cortex in higher forms. As for the greater loss of sensory and intellectual capacities and the slower recovery which appears after cerebral injuries in higher forms, they are restricted largely to man. The differences between the rat and the apes are scarcely greater than those between the apes and man. Both the sensory and intellectual capacities of the adult man are the result of years of training which have led to the establishment of countless

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habits having a definite structural basis. Cerebral injury may destroy a very great number of these, instead of the few which such an animal as the rat has formed, and the apparent loss of function will therefore be greater. Further, the rate of learning in the human adult depends largely upon the number and complexity of the habits already organized. When the latter are abolished the entire system must be built up slowly *de novo* before anything like an approach to adult performance is attained. The recent studies of reëducation in hemiplegia, aphasia, and apraxia show that the loss from cerebral lesions is never necessarily permanent in man and that an unlimited though slowly acquired vicarious functioning is possible. The difference seems to be one of degree rather than of kind. The rat loses less than higher forms after cerebral injury because he has less to lose and he seems to recover more rapidly chiefly because a little improvement brings him relatively nearer the standard of comparison (normality) than does the same amount of improvement in man.

Summary

1. Destruction of the frontal pole, including the motor area, is probably followed by an increase in general activity. Injuries to other parts of the cerebrum have no dynamogenic effect.

2. Learning at normal rate of either kinaesthetic- or visuo-motor habits is possible after the destruction of any given part of the cerebral cortex of the rat.

3. Destruction of cerebral nuclei also has no effect upon the rate of learning.

4. Within limits from 100 to 50 per cent of the cortex there is no relation between the amount of cerebral material functional and the rate of formation of complex habits.

5. In normal animals the habit of brightness discrimination is mediated by the occipital pole of the cerebrum (area striata) and by no other part of the cerebral cortex.

6. Extensive lesions to one corpus striatum are accompanied by paralytic symptoms and it is probable that the stimulable cortex and the corpora striata have alternative motor functions.

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REFERENCES

BARENNE, J. G. D. DE: Sensory localisation in the cerebral cortex. Q. J. Exp. Physiol., 1916, ix, 355–390.

BECHTEREW, W. v.: Die Funktionen der Nervencentra. Jena, 1909.

BOLTON, J. S.: Amentia and dementia: A clinico-pathological study. Part III. Dementia. Jour. Mental Sci., 1905, lii, 221–277 et seq.

BRODMANN, K.: Vergleichende Lokalisationslehre der Grosshirnrinde. Leipzig, 1909.

BURNETT, T. C.: Some observations on decerebrate frogs with especial reference to the formation of associations. Amer. J. Physiol., 1912, xxx, 80–87.

DODSON, J. D.: Relative values of reward and punishment in habit formation. Psychobiology, 1917, i, 231–276.

DUNLAP, K.: Internal Secretion in Learning. Psychobiology, 1917, i, 61–64.

FERRIER, D.: The functions of the brain. New York, 1876.

FRANZ, S. I.: On the functions of the cerebrum: I. The frontal lobes in relation to the production and retention of simple sensory-motor habits. Amer. J. Physiol., 1902, viii, 1–22.

FRANZ, S. I.: On the functions of the cerebrum: The frontal lobes. Archives of Psychol., 1907, no. 2, pp. 64.

FRANZ, S. I., AND LASHLEY, K. S.: The retention of habits by the rat after destruction of the frontal portion of the cerebrum. Psychobiology, 1917, i, 3–18.

GOLTZ, F.: Der Hund ohne Grosshirn. Arch. f. d. ges. Physiol., 1892, li, 570–515.

ISENSCHMID, R.: Zur Kenntnis der Grosshirnrinde der Maus. Abh. d. Königl. Preuss. Akad. d. Wiss., 1911, 1–76.

KAPPERS, ARIËNS. Further contributions on neurobiotaxis No. IX. An attempt to compare the phenomena of neurobiotaxis with other phenomena of taxis and tropism. The dynamic polarisation of the neuron. Jour. Comp. Neurol., 1917, xxvii, 261–298.

KEPNER, W. A., AND TALIAFERRO, W. H.: Reactions of Amoeba proteus to food. Biol. Bull., 1913, xxiv, 411–429.

LASHLEY, K. S.: Visual discrimination of size and form in the albino rat. Jour. Animal Behav., 1912, ii, 310–331.

LASHLEY, K. S.: The color vision of birds. I. The spectrum of the domestic fowl. Jour. Animal Behav., 1916, vi, 1–26.

LASHLEY, K. S.: The effects of strychnine and caffeine upon the rate of learning. Psychobiology, 1917, i, 141–170.

LASHLEY, K. S., AND FRANZ, S. I.: The effects of cerebral destruction upon habit formation and retention in the albino rat. Psychobiology, 1917, i, 71–140.

LEYTON, A. S. F., AND SHERRINGTON, C. S.: Observations on the excitable cortex of the chimpanzee, orang-utan, and gorilla. Q. J. Exp. Physiol., 1917, xi, 135–222.

LOEB, J.: Comparative physiology of the brain and comparative psychology. New York, 1900.

LUCIANI, L.: Human physiology, vol. iii. London, 1915.

METALNIKOW, S.: Contributions a l'etude de la digestion intracellulaire chez les protozaires. Arch. d. zool. exp. et gen., 1912, ix, 373–499.

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MEYER, M.: The fundamental laws of human behavior. Boston, 1911.
MONAKOW, C. v.: Die Lokalisation im Grosshirn. Weisbaden, 1914.
MORGAN, L.: Instinct and intelligence. London, 1912.
ROTHMAN, M.: Demonstration des Hundes ohne Grosshirn. (Ber. über. d. V. Kongress f. exper. Psychol.) Leipzig, 1912, 256–260.
SHARP, R. G.: Diplodinium Ecaudatum; with an account of its neuromotor apparatus. U. of Calif. Pub. Zool., 1914, xiii, 43–122.
SLONAKER, J. R.: Description of an apparatus for recording the activity of small mammals. Anat. Record, 1908, ii, 116–122.
SOLLIER, P.: Le Problèm de la Mémoire. Paris, 1900.
TAYLOR, C. V.: The Neuromotor system of Euplotes. Proc. Soc. Exp. Biol., and Med., 1909, xvi, 101–103.
WATSON, J. B.: Behavior. New York, 1914.
YOCUM, H. B.: The neuromotor apparatus of Euplotes patella. U. of Calif. Pub. Zool. 1918, xviii, 337–396.

PLATE I

The extent of lesions in animals which learned the double-platform box after operation. The figures are arranged in groups of four, representing the dorsal and lateral aspects of the hemispheres and a horizontal section at the level of the ventricals. The degenerated areas are shown in black. Other areas certainly non-functional are dotted.

FIGS. 1–6. Injury to one hemisphere only (group 2).

FIGS. 7–9. Occipital cases (group 3).

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PLATE 1

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PLATE II

The extent of lesions in animals which learned the double-platform box after operation, continued.

FIG. 10. Occipital case (group 3).

FIGS. 11–15. Parietal cases (group 4).

FIG. 16 (17). Frontal cases, identical (group 5).

FIGS. 18-19. Fronto-parietal cases (group 6).

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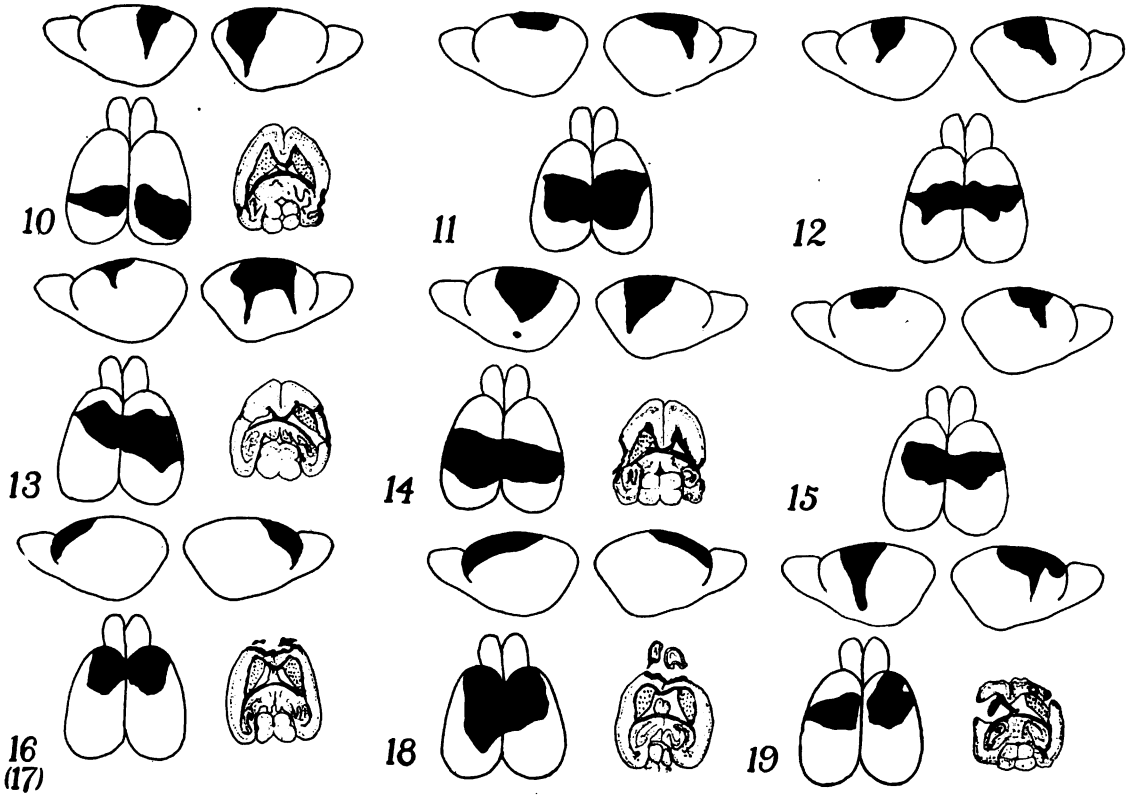

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PLATE III

The extent of lesions in animals trained in visual discrimination. Arranged as plate I.

FIGS. 1–4. Animals which formed the habit of brightness discrimination after operation.

FIGS. 5–6. Animals with destruction of the frontal pole which retained the visual habit after operation.

FIGS. 7–8. Animals with destruction of the parietal region which retained the habit of visual discrimination after operation.

FIG. 9. Occipital control.

FIGS. 10–12. Animals which lost the habit of visual discrimination after occipital lesions.

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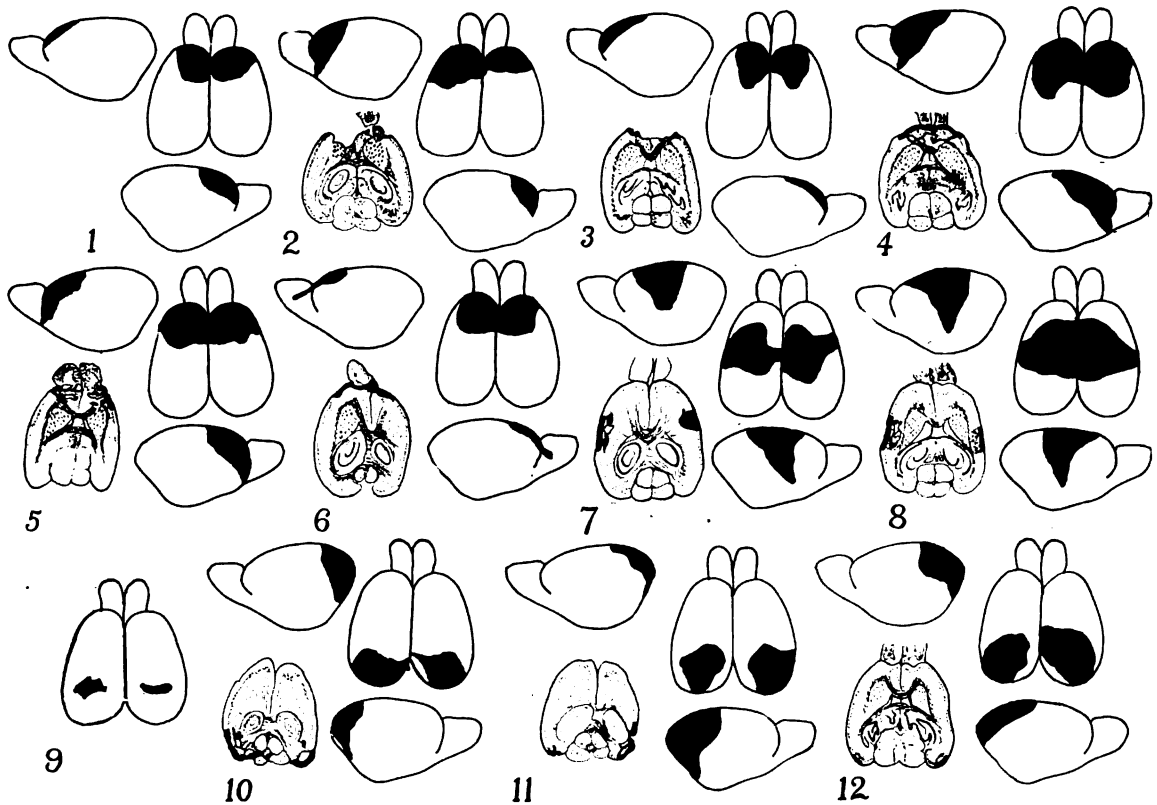

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PLATE IV

The extent of injury to the corpora striata and other subcortical structures in typical cases reported by Lashley and Franz ('17).

FIGS. 10*, 11*, 25*, 26*, 27*, 29*. Injuries to both corpora striata without paretic symptoms.

FIGS. 22*, 23*, 37*. Slight injuries to one corpus striatum without paretic symptoms.

FIGS. 36*, 38*, 39*. Extensive injuries to one corpus striatum with paresis of legs of opposite side.

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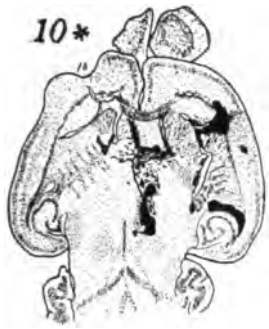

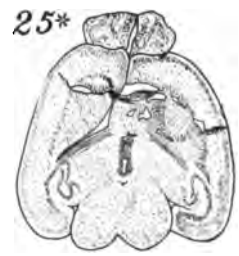

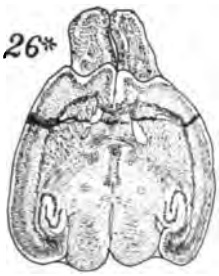

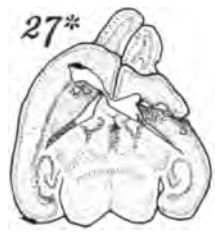

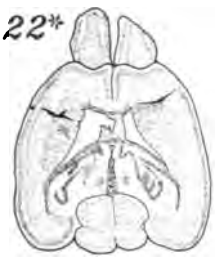

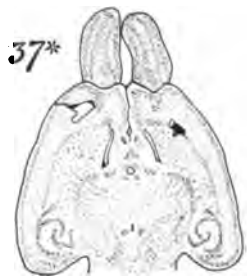

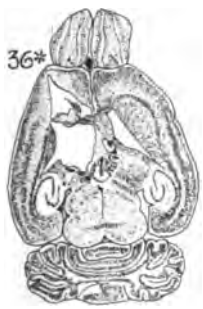

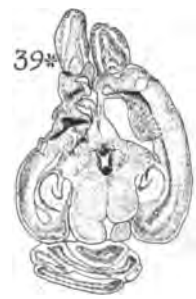

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THE RELATIVE STIMULATING EFFICIENCY OF CONTINUOUS AND INTERMITTENT LIGHT IN VANESSA ANTIOPA

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I. INTRODUCTION

Many investigators have studied the effect of intermittent light on the human eye, while but few have investigated its effect upon other eyes. In the work on the human eye the following four questions have been prominent:

1. Does intermittent light of high flash-frequency have the same stimulating efficiency as continuous light?

2. What is the flash-frequency at which the "flicker" of intermittent light becomes imperceptible?

3. What is the shortest perceptible time interval between two flashes of light?

4. What is the comparative apparent brightness of a given light acting for various lengths of time?

The first of these questions concerns Talbot's law. This law has been stated by Helmholtz (1896, p. 483) as follows:

If any part of the retina is excited with intermittent light recurring periodically and regularly in the same way, and if the period is suffi-

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ciently short, a continuous impression will result, which is the same as that which would result if the total light received in each period were uniformly distributed throughout the whole period.

Nearly all of those who have tested this law have concluded that it holds for the effect of intermittent light of high flash-frequency.

Plateau (1835, p. 457), Helmholtz (1896), Kleiner (1878, p. 542), Wiedemann and Messerschmitt (1888, p. 465), and Lummer and Brodhun (1896, p. 299–307) verified the law to within the range of their experimental error.

Fick (1863, p. 739) concluded that with intense illumination the action of intermittent light is stronger than it should be according to Talbot's law and that with very weak illumination the reverse is probably true.

Aubert (1864), however, maintains that the deviation from Talbot's law which Fick found is of the same order of magnitude as his experimental error and that therefore his results verify the law to within the limit of accuracy of his experiments.

Ferry (1893, p. 338) verified the law for white light but found quite large errors when the light transmitted through the rotating sectored disk was of a bluer quality than that incident on the other side of the photometer screen.

Hyde (1906, pp. 1–32), however, as the result of an extremely thorough piece of work, came to the conclusion that Talbot's law, in its application to a rotating sectored disk, holds for white light for all total angular openings between 288 and 10 degrees to within a possible error of 0.3 per cent, which he states probably expresses the limit of accuracy of his experiments. He also concluded that Talbot's law is verified for red, green, and blue light though not to such a high accuracy as for white light.

Parker and Patten (1912, pp. 22–29) on the contrary concluded that intermittent light is measureably less efficient as a stimulus for the eye than continuous light. Pfund, however, points out that Parker and Patten's methods were at fault and says (1914, p. 117): "It is therefore established by methods that are entirely free from objections that Talbot's law holds to a high degree of accuracy for the human eye."

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Most of the investigators mentioned above used a fairly high flash-frequency. Lummer and Brodhun used a frequency of interruption of from 27 to 200 per second. Parker and Patten tested the effects of flash-frequencies of from 36 to 196 per second but in most of their tests used a flash-frequency of over 116 per second. Hyde states that in his work no attention was paid to the number of flashes per second except to prevent all possibility of a flicker. The flash-frequencies used by the other workers are not available.

The flash-frequency at which the "flicker" of intermittent light becomes imperceptible, the "Verschmelzungsfrequenz" of von Kries, depends upon the illumination. Baader (1891) states that at a flash-frequency of 18.96 per second the flicker of intermittent light of an illumination designated as "1" cannot be perceived and that the fusion-frequency rises gradually with increasing illumination until at an illumination of "1800" the frequency of interruption must be 50.24 per second before the flicker disappears. According to Helmholtz the fusion-frequency in strong lamp light is 24 per second, while that in full moon light is only 10 per second. Kleiner (1874) also concluded that the frequency at which the flashes of light fuse into a continuous impression varies with the illumination.

The fusion-frequency has also been shown to depend on the condition of adaptation of the eye. Schaternikoff (1902) and Charpentier (1887) state that with increasing dark adaptation the fusion-frequency rises from about 10 to about 17 per second if weak lights are used. The reverse is true if strong lights are used, according to von Kries and Schaternikoff. In this connection it is interesting to note that the fusion-frequency for totally color-blind individuals does not rise higher than 20 per second (von Kries, 1905, p. 255).

The fusion-frequency depends also upon the relative duration of the periods of light and darkness in intermittent light according to Dunlap, who maintains (1915, p. 230) that for the dark adapted eye the fusion-frequency rises from 28.17 to about 50 flashes per second, as the ratio between the duration of the flashes and of the interval between the flashes changes from 35/1 to

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1/1, and decreases from about 50 to 34.04 flashes per second, as the ratio between the duration of the flashes and of the interval changes from 1/1 to 1/35. Schenck (1897, p. 54) states that if in intermittent light a grey period is interposed between the periods of darkness and of light the fusion-frequency increases, while Helmholtz (1896, p. 489) and Plateau maintain that the fusion-frequency is not affected by changes in the relative duration of the light and dark periods of intermittent light.

According to von Kries the results of Baader and Schenck show that in intermittent light consisting of alternate strong and weak flashes the fusion-frequency depends on the relative brightness of the flashes. As the relative brightness decreases the fusion-frequency decreases no matter whether the average illumination is unchanged or increases.

The first to investigate the shortest perceptible time-interval between two flashes of light was Exner who, working with electric sparks, found visual time-thresholds at 44/1000 seconds at 280 mm. distance and 21/1000 seconds at 640 mm. Weyer (1899), who also used electric sparks, states that the visual time-threshold is only 12/1000 seconds. If a series of stimulations is used, he holds that according to adaptation and other conditions, the flicker-threshold varies from 25/1000 to 87/1000 seconds and the threshold for separation of a series varies from 42/1000 to 105/1000 seconds.

Basler (1911), using a rotating black disk on which were painted white sectors states that the time-threshold is about 40/1000 seconds with two visual stimulations and for a series of stimulations it is about one-third as much. Dunlap, whose careful investigations have shown the difficulties of this problem, states that with two stimulations the time threshold decreases with increase in the length of the flashes and that this decrease seems to be altogether a function of the duration of the first flash. He also comes to the following conclusions as to the visual time-threshold (1915, pp. 247–248):

1. The effects of brightness of the light are variable, depending on the other factors in such a way that no conclusion can be drawn as yet concerning their effects.

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2. The threshold is lower for the light-adapted eye than for the dark-adapted eye. This holds, at least, for certain light-adaptations.

3. The threshold is lower for an interval marked by flashes added to a continuous stimulation, than flashes in a dark field. This holds for a wide range of constant illumination, the threshold varying usually with the brightness of the constant illumination up to the point where the additions lose in distinctness.

The fourth question mentioned above, as to the relative apparent brightness of light acting for various lengths of time, has been answered differently by different investigators, who have also revealed several other interesting phenomena. The chief attempts in this direction have centered around the "action-time" of light, i.e., the time during which light must act in order to produce its maximum effect in point of apparent intensity. Exner (1868, p. 601), Martius (1902), and McDougall (1904, pp. 151–189) maintain that the action-time of light varies inversely with the illumination, while Swan maintains that it does not. According to McDougall the action-time of light is 0.2 seconds when the stimulus is so weak as to be barely perceptible, and decreases to 0.03 seconds when the intensity of the stimulus is sufficiently increased. He also states that the action-times of red, green, and blue lights of the same intensity differ but little or not at all. Kunkle (1874, p. 197) however holds that the action-time of red, green, and blue light differs.

The effect of light acting for longer than its action-time has been investigated by McDougall (1904), who concludes that when light of a certain illumination acts upon the human retina for longer than its action time the apparent brightness of the light remains equal to, and then becomes less than that of the same light acting for only its action-time. McDougall also states that when two lights of the same illumination act for different periods both however for longer than the action-time, the one of the shorter duration seems to the human eye to be the brighter.

The effect of light acting for less than its action-time seems to vary directly with the duration of the action of the light. This contention is supported by the work of McDougall, Swan (1849), Bloch (1885), and Charpentier (1887), McDougall says (1904, p. 177):

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I am strongly disposed to believe that the law that the intensity of the sensation varies directly with the duration of the action of the light of given intensity holds good for all durations less than the action-time of the light.

In this connection and in view of the facts described in this paper it is also of especial interest to note the following results. Flickering lights of certain flash-frequencies seem to have a greater stimulating efficiency for the human eye than have the lights of the same illumination but of different flash-frequencies. von Kries says (1905, p. 232):

Hat man auf einer rotierenden Scheibe Ringe mit verscheidenen Zahlen schwarzer und weisser Sektoren, so bemerkt man bei passenden Rotationsgeschwindigkeiten, dass ein stark flimmernder Ring im ganzen beträchtlich heller erscheint als ein vollkommen stetig gesehener. Brücke gab an, dass bei einer Frequenz der Reizanstösse von etwa 17,5 in der Sekunde die Helligkeit am grössten erscheine.

Hyde and Cady (1906, pp. 415–437) confirm these results, for in an investigation of the mean horizontal intensity of incandescent lamps by the rotating lamp method they found that when certain types of incandescent lamps were rotated at three revolutions per second a flickering light was produced, which to some observers appeared of an intensity 4 per cent too high and to others 3 per cent too low. Ferry also confirms these results for he says (1894, p. 344): "But it was noticed that if the (rotating sectored) disc did not revolve rapidly enough to produce a perfectly steady illumination of the photometer screen, *more* light appeared to go through the sectored disc than theoretically should." Thus, apparently to the human eye intermittent light in which the flashes of a certain duration are perceptible seems stronger than it really is.

The effect of intermittent light upon organisms other than man has been studied but little and most of the investigators have been interested in the first of the problems mentioned above, i.e., the applicability of Talbot's law. Loeb, Ewald (1914), and Wastenays (1917) state that this law holds for Eudendrium; Ewald (1914) maintains that it holds for Daphnia; Loeb and

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Northrup (1917) that it holds for Balanus larvae; Patten (1915) that it holds for blow-fly larvae and Clark (1913), Blaauw (1909), Fröschel (1910), and Fitting (1905) that it holds for certain plants.

In these tests, as in those previously mentioned, the flash-frequency was usually high. For example, in the work of Loeb and Northrup the intermittent light was of a flash-frequency of from 50 to over 83 per second, while Patten used a frequency of interruption of 115 per second.

Apparently Ewald alone has investigated the effect upon an animal other than man of intermittent light of low flash-frequency. He states (1914) that Daphnia orients in intermittent light as it does in continuous light independently of whether the frequency of interruption is 1 or 30 per second, but that, in reference to the orientation of the eye of Daphnia, intermittent light of lower frequencies of interruption has a weaker effect than continuous light. He says (1913, p. 237):

> In some cases I got a marked reaction of the eye on change from constant to intermittent light of equal energy when the speed of the sector-wheel was about one-tenth of a second per revolution. The deviation becomes more marked the slower the speed.

In harmony with the results described above are those obtained in certain investigations on the effect of light upon photographic plates. Abney, Kron, Helmic, and Newcomer state that the blackening of photograpic plates by light does not depend solely upon the total energy received. Helmic (1918, p. 374) maintains that it "is dependent upon the rate of flow of energy, with total energy constant; and that for each brand of plate and quantity of total energy there is a maximum blackening given by a certain rate of flow of energy." According to Helmic, Abney (1901, p. 395) and Kron (1913, p. 755) reached the same conclusions. Moreover, Newcomer (1919, p. 243) asserts that intermittent light produced by a rotating sectored disk with a 72 degree aperture produces less blackening of a photographic plate than continuous light of equal total energy. He does not state the flash-frequency used but it was presumably low.

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In view of the slight information available as to the effect of intermittent light upon organisms other than man it seemed desirable to make a thorough study of this question in some of the lower forms. The mourning cloak butterfly, Vanessa antiopa, was chosen because its reactions to light are fairly well known and because it lives well in captivity and is not difficult to obtain in large quantities.

Before entering upon a discussion of these experiments I wish to express my very sincere appreciation of the kindness of Professor S. O. Mast whose many suggestions and unselfish aid have made this work possible. It is a pleasure also to acknowledge my indebtedness to Dr. H. E. Howe for the loan of apparatus and for other kindnesses and to Mrs. O. F. Hiser, who supplied me with the larvae from which the butterflies were reared.

II. METHODS

The butterflies used were all reared in the laboratory from larvae secured from both the June and August broods in Iowa. They were kept in a large wire cage, which was protected to some extent by a tree which partially shaded it. They were fed upon decaying fruit and a weak solution of maple syrup in water. The wings of those butterflies used in the tests were clipped to prevent their escape. This was not injurious, for animals with clipped wings live and thrive as well as normal specimens, and they behaved in the same manner.

Two slightly differing methods were used. Those used in all of the experiments, except those described in section V of this paper, were as follows. The observations were all made on a table in a dark room in a field of light composed of two small horizontal beams, produced by two 100 Watt Edison gas filled lamps, so situated that the beams crossed at right angles. The lamps were mounted in front of a small opening in a light proof box that was painted dead black inside. By means of screens the light from these lamps was so cut down as to produce sharply defined beams of the size desired. These beams were the only light in the room, and this was in large part absorbed

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by means of black cloth which covered the walls of the room. There was consequently very little light in the room aside from that in the beams. One of the lamps was in an adjoining room, and the beam from it passed through an aperture in the intervening wall. The other lamp was placed upon a support so that its position could be easily changed. In some of the experiments the light in one of the beams was intermittent. The intermittent light was produced by means of a rotating sectored disk connected with a small motor which was run on Edison storage batteries. The motor and sectored disk as well as the lamp were all placed in an adjoining room, separated by a 30 cm. thick brick wall from the room in which the experiments were performed, so as to reduce vibration as much as possible. The flash-frequency and the duration of the flashes and of the dark periods between the flashes of the intermittent light was controlled by using different disks with apertures of various sizes and by varying the number of revolutions per second.

Not only was the behavior of the animals observed closely by the investigator but the butterflies themselves were forced to make permanent records of their own behavior. This was done by allowing them to walk on sheets of paper (15 by 21 cm.) which had been covered with soot from an oil lamp. The tracings made by the insects were made permanent by means of a coat of shellac. Tests showed that the behavior was not affected by the soot. Upon the sheets bearing the tracings of the insects were also marked the limits and direction of the beams of light. Individuals were given a varying number of trials under the same conditions. Sometimes 5 were given and at other times more. An insect was never allowed to make more than 10 tracings on one sheet. Where more trials were given other sheets were used. Usually an animal was given 5 consecutive trials from a point facing one source of light. It was then given 5 trials from a point facing the other source. In analyzing the tracings made a line was drawn bisecting the angle made by the rays of light in the center of each beam. The angle that each tracing made with this line was then measured. If, for example, the tracing was at an angle of 10 degrees with the line bisecting the angle

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between the beams, it was marked "plus" or "minus" 10 degrees, depending upon whether the butterfly deflected toward the right or the left. The intermittent light was always to the right. Deflection toward it was consequently always "plus" (see figure 1).

The methods used in the experiments described in section V of this paper were similar to those described above except that the light conditions were slightly different and other means of recording the movements of the animals were used. As light sources two small 36 candle power automobile lamps run on storage batteries were used. The motor, sectored disk, and lamps were all in the same room as that in which the experiments

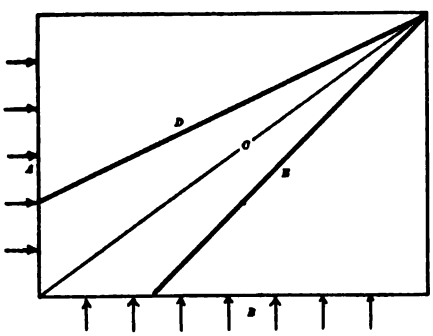

FIG. 1. Diagram to show methods used in recording the reactions of Vanessa in a field of light composed of two horizontal beams crossing at right angles. (Reduced by three-fourths). Arrows, direction of rays of light; *A*, intermittent light; *B*, continuous light; *C*, line bisecting the angle made by the two beams; *D*, path of butterfly making an angle of +10 degrees; *E*, path of butterfly making an angle of −10 degrees.

were performed. The vibration caused by the motor was very slight and did not affect the results, as was shown by tests. Instead of using sheets of paper covered with soot to record the movements of the animals, very fine charcoal dust was sprinkled on the sheets mentioned above with a pepper shaker. After an insect had moved across a sheet so prepared a pencil was drawn along the trail left by the organism. After removal of the charcoal the sheet with no further treatment bore a permanent record of the reactions of the animal.

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The candle power of the lamps used in the experiments was measured by means of a Sharp-Millar photometer by Prof. H. E. Howe of the Physics department of Cornell University, who also approved the arrangement of the experiments. The candle power of the lamps used in the experiments described in sections II, IV, VI, and VII was not measured until about a year after the experiments were performed. During the interim they were not used however and the depreciation due to age was probably slight.

III. ORIENTATION IN CONTINUOUS LIGHT FROM TWO SOURCES

Since the object of the experiments described in this paper was to ascertain the effect of intermittent light on orientation in Vanessa it was first necessary to know the effect of continuous light on orientation. This was ascertained in the following manner. Two horizontal beams of light were arranged, as described previously, so that they crossed at right angles. A sheet of paper covered with soot from an oil lamp was placed in the field of light common to the two beams, so that it was illuminated by an equal amount of light energy from each of the sources. A butterfly with clipped wings was picked up in the fingers and gently placed so that it faced a point about halfway between the two sources, care being taken to avoid excessive stimulation by handling. The insect moved across the sheet of paper leaving a trail in the soot. When it had completed this trial it was immediately picked up, placed again at about the same starting point, and allowed to complete another trial. In the same way 20 consecutive trials were given under these conditions. The direction of the rays of light was then marked on the sheet, and it was removed to be later coated with shellac, as described above. Similarly the same individual was given 20 consecutive trials with the relative illumination of the two beams of light in the following ratios respectively, 3 to 4, 2 to 1, and 1 to 4. An interval of at least thirty minutes separated each of the series of tests under the above four different light conditions. Thirteen butterflies were used, most of them being given all of the tests described above. The results obtained are given in table 1.

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Table 1 shows that when the illumination in the two beams was equal the average angle made by 11 butterflies was +0.78 degrees, indicating that the butterflies tended to move in general toward a point halfway between the two sources of light; and that when the illumination in one beam was greater than that in

TABLE 1

Orientation in continuous light from two sources. The numbers indicate the degree of deflection from a line bisecting the 90 degree angle between the two beams. "Plus," deflection toward right beam. If beams are of unequal illumination the stronger one is to the right of the insect. "Minus," deflection toward other beam

DESIGNATION OF BUTTERFLIES	AVERAGE ANGLE OF DEFLECTION (20 TRIALS FOR EACH AVERAGE), IN FOLLOWING ILLUMINATION							
	Right beam	Left beam	Right beam	Left beam	Right beam	Left beam	Right beam	Left beam
	14 m.c.	3.5 m.c.	14 m.c.	7 m.c.	14 m.c.	10.5 m.c.	14 m.c.	14 m.c.
13	−10.7		+12.6		−13.0		−13.7	
6	+18.55		+8.25					
8					−10.8		−9.0	
25	+13.10		+9.3		+9.9		+8.3	
23	+16.2		+17.9		+8.65		+7.5	
21	−7.8		−2.7		+1.55		+2.2	
16	+9.45		+14.1		−5.6		+4.57	
15	+4.05		+0.6		−6.4		−4.1	
14	+14.4		+12.45		+6.0		+5.35	
12	+17.35		+6.02		+17.8		+16.3	
11			−4.5		−14.25			
9	−5.1		+5.35		−10.05		−13.85	
7	+10.45		+4.95		+12.8		+5.1	
Total average angles.....	+7.27		+7.03		−0.28		+0.78	

the other they moved toward a point nearer the source of stronger light. This conclusion is evident from a comparison of the total average angles made in beams differing in relative illumination. The only apparent exception is the average angle made in two beams one of which was three-fourths as strong as the other. Although the total average angle made by all of the butterflies in this test was only −0.28 degrees, 63.6 per cent or 7 out of the 11 animals tested under the two conditions, first, in two beams of equal illumination and second, in two beams, one three-fourths

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as strong as the other, deflected more toward the stronger beam in the latter test than they deflected toward either beam in the former test. An examination of table 1 shows also that the butterflies exhibited great variation in their reactions. For example, the average angles made by the different insects in two beams of equal illumination varied from −13.85 to +16.3 degrees. The causes of this variation are discussed later. It is clear however that in spite of great variation Vanessa when exposed to two beams of continuous light which cross at right angles tends in general to move toward a point between the sources, the location of which depends upon the relative illumination in the beams. This seems to indicate that the effect of light on orientation varies with the illumination.

This conclusion is supported by the results obtained with many other organisms by Mast (1907 and 1911), Ewald (1913), Patten (1914), and Loeb and Northrup (1917).

IV. VARIATION IN REACTIONS TO LIGHT

As stated above, Vanessa shows great variation in its reactions to light. This is evident both in its reactions in continuous and in intermittent light. Different individuals react differently, as is shown by an examination of tables 1, 3, and 5. A given insect also at times may show great variation in successive trials under the same conditions. This is well illustrated in table 2.

Table 2 shows that with the light from the two sources equal the angles made in the individual trails varied from −14 to +21 degrees; that with the illumination in one beam three-fourths that in the other the angles of the individual trails varied from −21 to +30 degrees; that with the illumination in one beam twice that in the other the angles of the individual trails varied from 0 to +37 degrees; and that with the illumination in one beam four times that in the other the angles made in the individual trails varied from 0 to +32 degrees. It is consequently evident that Vanessa may show great variation under the same conditions when reacting to two sources of continuous light. In spite of this variation the average angle made increased, in gen-

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eral, as the difference between the illumination of the two beams increased, for, as the last line in table 2 shows, the average angle made in 20 trials when the light from the two sources was equal

TABLE 2

Showing the variation exhibited by a specimen of Vanessa (butterfly 14) in successive trials under the same conditions. The numbers indicate the degree of deflection from a line bisecting the 90 degree angle between two beams of continuous light. "Plus," deflection toward right beam. If beams are of unequal illumination the stronger one is to right of the insect. "Minus," deflection toward other beam

SERIAL ORDER OF TRIALS	ANGLES IN DEGREES OF EACH OF THE 20 TRIALS							
	Illumination in meter-candles							
	Right beam	Left beam	Right beam	Left beam	Right beam	Left beam	Right beam	Left beam
	14	14	14	10.5	14	7	14	3.5
1	+19		+12		+10		+18	
2	+19		+9		+6		+18	
3	+19		+8		+8		+32	
4	+11		+8		+8		+21	
5	+11		0		+7		+26	
6	0		0		0		+22	
7	0		0		+8		+13	
8	0		−11		+7		+32	
9	0		−21		0		+28	
10	0		+0.5		0		+22	
11	+13		+10		+16		0	
12	+21		+15		+37		0	
13	+7		+30		+36		+9	
14	+8		+22		+12		+9	
15	0		+20		0		+10	
16	0		0		+36		+10	
17	0		0		+22		0	
18	0		+6		+22		+4	
19	−14		+6		+7		+7	
20	−7		+6		+7		+7	
Average angle made in 20 trials.....	+5.35		+6.02		+12.45		+14.4	

was +5.35 degrees; when the illumination in one beam was three-fourths that in the other the average angle of 20 trials was +6.02 degrees; when the illumination in one beam was twice that in the other the average angle was +12.45 degrees; and when

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the illumination in one beam was three times that in the other the average angle was +14.4 degrees. This conclusion is supported by the data presented in table 1, as is shown in the preceding section. This seems to show that, in spite of the enormous variation described, conclusions based upon averages of numerous tests are trustworthy. We have consequently in the work described in the following pages continuously dealt with such averages.

The cause of the variations mentioned is in itself an exceedingly interesting problem. Why, for example, does a butterfly under given conditions deflect zero degrees in one test and +36 degrees in the following test? We assume that this change is due to internal changes of some sort or other. What these changes are is not known, but we shall demonstrate later that they are at times, at least, associated with the past experiences of the individuals involved.

V. RELATIVE STIMULATING EFFICIENCY OF INTERMITTENT AND CONTINUOUS LIGHT

In the following experiments it was found that at certain flash-frequencies the stimulating effect of intermittent light is greater than that of continuous light of equal illumination; at other flash-frequencies it is less than that of continuous light; and at still others it is equal to that of continuous light.

In these experiments the butterflies were exposed at the intersection of two horizontal beams of light which crossed at right angles, as in the preceding experiments. The paths of the insects were recorded by allowing them to walk over sheets of paper covered with charcoal dust, as described previously. Each one of 10 animals was given the following tests. An individual was first given 5 trials in two beams of continuous light of equal illumination. Before each trial the insect was placed so that it faced a point about halfway between the two sources. As soon as it had walked across the field of light it was gently picked up in the fingers and placed again at the starting point preparatory for another trial. Care was taken not to pinch the animal or to

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stimulate it by handling. When the five trials were completed a water glass was inverted over the insect to prevent its escape. A pencil was then drawn along the trails made on the record sheet: the direction of the rays of light was marked on it: and it was replaced by a fresh sheet. A sectored disk with one-fourth removed, rotating at the rate of 100 revolutions per second, was next interposed in one of the beams, and the illumination in the other beam was decreased by changing the position of its source until the amount of light received from the two sources was equal, as in the preceding tests, that from one, however, being intermittent and that from the other continuous. The butterfly was then given 5 consecutive trials just as in the preceding test. After an interval of about five minutes, 5 more consecutive trials were given with the flash-frequency of the intermittent light 60 per second. Similarly, tests were made with the flash-frequency of the intermittent light 50, 40, 30, 20, 10, 5, and 2 per second respectively. The results obtained may be illustrated by a detailed description of a typical experiment.

In this experiment the butterfly was first given 5 trials in two beams of continuous light of equal illumination. It deflected somewhat toward the source to its right, making an average angle of +12.3 degrees with the line bisecting the angle made by the two beams of light. It was then exposed to two beams of equal illumination, one of them being of continuous and the other of intermittent light. The flash-frequency of the intermittent light was 100 per second. The organism made the same average angle that it had made in two beams of continuous light of equal illumination, +12.3 degrees. The insect was next given tests in intermittent light of flash-frequencies of 60, 50, 40, and 30 per second. It made the following average angles respectively: +4.4, +6, +19.8, and +15.6 degrees, thus reacting to intermittent light of high flash-frequency in general as it did to continuous light. A test in intermittent light of a flash-frequency of 20 per second followed. The butterfly made a much larger average angle, +29.7 degrees, indicating that the stimulating efficiency of intermittent light of this flash-frequency was greater than that of continuous light. The effect of intermittent light of

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a frequency of interruption of 10 per second was next tested and was found to be about equal to that of continuous light, for the average angle made was +12.8 degrees, about the same as that made in two beams of continuous light of equal illumination. Tests in light of flash-frequencies of 5 and 2 per second were next given. The angles of deflection made were −1.4 and −23.4 degrees, respectively, showing that the stimulating efficiency of intermittent light of these flash-frequencies was less than that of continuous light.

Consequently, the reactions of this individual indicate: (1) that intermittent light of a flash-frequency of 20 per second has a higher stimulating efficiency in Vanessa than continuous light, i.e., a given amount of intermittent light has a greater stimulating effect than the same amount of continuous light, (2) that intermittent light of flash-frequencies of 5 and 2 per second has a lower stimulating efficiency than continuous light, and (3) that the stimulating efficiency of intermittent light of flash-frequencies of 100, 60, 50, 40, 30, and 10 per second is approximately equal to that of continuous light. These conclusions are in general strongly supported by the results of all of the tests made which are presented in tables 3 and 4.

By referring to table 3 it will be seen at once that the results presented in the last two columns show that the efficiency of intermittent light of low flash-frequency (5 and 2 per second) is clearly lower than that of continuous illumination. Only in two individuals was there any indication of an exception to this; "H" and "C" both deflected less toward intermittent light of 5 flashes per second than they did toward continuous light of the same illumination. In all of the other flash-frequencies the variation in deflection in different individuals is so great that the meaning of the results is not immediately evident. For example, in specimen F the greatest stimulating efficiency appears to have been at 100 flashes per second; in B at 50; in L at 40; in J at 10; in C and in E at 30; and in N, I, G, and H at 20. A thorough analysis of these results shows however that in spite of this variation the results are quite clear. Such an analysis is presented in the last line in table 3 and in table 4.

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Examination of these shows that the stimulating efficiency of intermittent light of a flash-frequency of 20 per second is higher than that of continuous light. This statement is based on two facts: first, the average angle of deflection of all 10 insects in intermittent light of 20 flashes per second (+7.83 degrees) was greater than that made in continuous light; and second, 80 per

TABLE 3

Comparative stimulating efficiency of continuous and intermittent light. The numbers indicate the degree of deflection from the line bisecting the angle between two beams crossing at right angles. "Plus," deflection toward the source of intermittent light, or toward the right if both beams are continuous. "Minus," deflection toward the source of continuous light, or toward the left if both beams are continuous. Each figure given is the average angle of 5 trials

DESIGNATION OF INSECTS TESTED	LIGHT IN BOTH BEAMS CONTINUOUS: ILLUMINATION IN BOTH, EQUAL, 14 m.c	ONE BEAM, CONTINUOUS; THE OTHER, INTERMITTENT: ILLUMINATION IN BOTH, EQUAL, 3.5 M.C.								
		Flash-frequency per second								
		100	60	50	40	30	20	10	5	2
		Angles of deflection								
N	+12.3	+12.3	+4.4	+6.0	+19.8	+15.6	+29.7	+12.8	−1.4	−23.4
L	+4.8	−2.2	+7.2	−9.6	+12.6	+5.2	+0.2	−13.8	−27.0	−48.6
J	+5.2	+10.4	+20.4	+30.0	+8.4	+18.6	+16.3	+21.1	−36.0	−35.6
I	+5.6	+12.8	+12.8	0	+3.4	+2.0	+14.0	−7.8	−10.2	−29.2
H	−5.7	−1.0	+2.4	0	0	−3.6	+7.6	0	−1.6	−39.0
G	−7.4	−11.6	−10.0	−12.0	−9.8	−12.2	−6.8	−27.6	−23.2	−32.6
F	+4.4	+13.0	+9.6	+4.8	+1.6	+10.2	+8.7	−38.4	−25.4	−24.8
E	+16.4	+15.4	+13.8	+13.8	+15.8	+24.8	+19.4	0	−12.4	−7.2
C	+8.2	+18.4	+13.8	+20.4	+15.6	+32.0	+13.0	+39.8	+31.2	−7.6
B	+14.6	−1.4	−2.6	+11.2	+8.0	−2.8	−23.8	−15.5	−16.0	−14.0
Average	+5.84	+6.61	+7.18	+6.46	+7.54	+8.98	+7.83	−2.94	−12.2	−26.2

cent, or 8 out of the 10 insects tested, deflected more toward the source of intermittent light of this flash-frequency than they did toward either source when tested in two beams of continuous light of equal illumination.

Similarly, the stimulating efficiency of intermittent light of a flash-frequency of 30 per second seems to be greater than that of continuous light, for 70 per cent, or 7 out of the 10 insects tested

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deflected more toward the source of intermittent light of this flash-frequency than they did toward either source when tested in two beams of continuous light of equal illumination. Moreover, the average angle of deflection of all 10 insects in light of 30 flashes per second (+8.98 degrees) was greater than that made in continuous light.

The efficiency of light of 100 flashes per second is apparently equal to that of continuous light for the average angle of deflec-

TABLE 4

Relative stimulating efficiency of intermittent light of various flash-frequencies and continuous light of equal illumination. This table is based upon the data given in table 3

FLASH-FREQUENCY OF INTERMITTENT LIGHT PER SECOND	PERCENTAGE OF BUTTERFLIES TESTED IN WHICH THE STIMULATING EFFICIENCY OF INTERMITTENT LIGHT WAS		
	Greater than that of continuous light	Less than that of continuous light	Equal to that of continuous light
100	50	40	10
60	60	40	0
50	40	60	0
40	50	50	0
30	70	30	0
20	80	20	0
10	40	60	0
5	20	80	0
2	0	100	0

tion of all 10 insects made in both sorts of light was approximately equal, being +6.61 degrees in the former and +5.84 degrees in the latter. This conclusion is supported by the fact that 50 per cent of the 10 animals deflected more toward the source of intermittent light than they deflected toward either source when tested in two beams of continuous light of equal illumination. Forty per cent deflected less, and 10 per cent deflected to an equal extent. It is therefore evident that for Vanessa the stimulating efficiency of intermittent light of a flash-frequency of 100 per second is approximately equal to that of continuous light.

A study of table 4 indicates that the same conclusion is to be drawn as to the stimulating efficiency of intermittent light of

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flash-frequencies of 60, 50, 40, and 10 per second. However an examination of the averages presented in the last line in table 3 indicates that the stimulating efficiency of intermittent light begins to rise at a flash-frequency of about 60 per second, reaches a maximum at 30 per second, and then declines. According to these figures the efficiency of light of 10 per second is less than that of continuous light. If this is true the stimulating efficiency of intermittent light of a flash-frequency of somewhere between 10 and 20 per second is equal to that of continuous light. In the absence of more data it is probably wiser to assume that the conclusions drawn from table 4 are more nearly correct than those drawn from the last line in table 3.

The conclusion stated above that the stimulating efficiency of intermittent light varies with the flash-frequency is further supported by the results presented in table 5. This table shows that the total average angles of the butterflies tested in intermittent light produced by a sectored disk with one-fourth removed and of the following flash-frequencies: 30, 20, 15, 10, 5, and 2 per second were respectively: +15.38, +7.67, −4.16, +0.84, −14.67, and −22.76 degrees. The angles of deflection of the butterflies tested in light produced by a disk with one-half removed and of the following flash-frequencies: 30, 20, 15, 10, 5, and 2 per second were respectively: +10.75, +6.73, −0.71, +0.92, −6.58, and −15.67 degrees. The angles of deflection of the butterflies tested in light produced by a disk with three-fourths removed were respectively +1.37, +0.11, +1.31, +0.07, −2.15, and −5.26 degrees. Thus, in all three sorts of light the angles of deflection decrease in general as the flash-frequency decreases from 30 to 2 per second. These angles are averaged from various numbers of insects but this fact does not invalidate the conclusions drawn from these average angles, for, while these insects showed great variation, an examination of the angles made by the different individuals leads to the same deduction. Moreover, the conclusions drawn thus far in this section are supported by numerous results presented in the following sections.

The statement that intermittent light of flash-frequencies of 20 and 30 per second has a higher stimulating efficiency in

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TABLE 5

Relation between stimulating efficiency and flashes of low frequency. The numbers indicate the degree of deflection from the line bisecting the angle between two beams of equal illumination, crossing at right angles. One beam, continuous; the other, intermittent. "Plus," deflection toward source of intermittent light; "minus," deflection toward source of continuous light. This table is the summary of the results obtained in 3400 trials, each number given being the average angle made in 20 trials, except those for butterflies 13 and 8, which are the average of 10 trials

DESIGNATION OF BUTTERFLIES	AVERAGE ILLUMINATION IN EACH BEAM IN METER-CANDLES	PORTION OF SECTORED DISK REMOVED	FLASH-FREQUENCY PER SECOND					
			30	20	15	10	5	2
			Angles of deflection					
13	3.5	1/4	+9.725	+9.725		−0.4	−25.3	−32.25
	7.0	1/2	+0.7	−4.5		−15.2	−16.2	−24.3
	10.5	3/4	−12.6	−13.45		−13.7	−17.3	−17.4
6	3.5	1/4	+27.65	+14.9		+4.2	−12.0	−29.5
	7.0	1/2	+14.85	+15.7		+18.15	−2.63	−39.8
	10.5	3/4						
8	10.5	3/4	−9.22	−9.5		−18.9	−23.8	−15.6
25	3.5	1/4	+11.6	+12.55		+2.45	−18.15	−22.3
	7.0	1/2	+11.65	+14.65		+1.9	−3.05	−13.15
	10.5	3/4	+4.95	+6.75		−1.4	+5.25	−6.95
23	3.5	1/4	+9.7	+5.2		−0.25	−17.1	−17.5
	7.0	1/2	+12.75	+13.85		+10.45	+ 3.95	+4.4
	10.5	3/4	+13.425	+4.6		+1.55	+6.0	+5.0
21	3.5	1/4	+29.5	+7.2		+1.75	−9.25	−23.95
	7.0	1/2	+12.25	+0.35		−6.75	−14.5	−28.35
	10.5	3/4	+4.05	+8.75		+8.4	−0.2	−6.25
20	3.5	1/4	+4.15	+11.8		−0.365	−22.3	−30.3
	7.0	1/2	+12.35	+11.35		+7.9	−5.55	−19.25
	10.5	3/4	+7.65	+3.75		+17.1	+5.6	+1.2
16	3.5	1/4		+8.9	−4.6	+8.95	−12.75	−21.4
	7.0	1/2		+8.9	+8.25	−2.15	−2.8	−6.2
	10.5	3/4		+5.9	−5.1	+0.7		−8.3
15	3.5	1/4		−7.1	−9.05	−0.3	−15.25	−18.2
	7.0	1/2		−1.2	+0.3	−6.1	−7.95	−15.05
	10.5	3/4		−3.45	−2.6	−4.65		−6.95

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TABLE 5—*Concluded*

DESIGNATION OF BUTTERFLIES	AVERAGE ILLUMINATION IN EACH BEAM IN METER-CANDLES	PORTION OF SECTORED DISK REMOVED	FLASH-FREQUENCY PER SECOND					
			30	20	15	10	5	2
			Angles of deflection					
14	3.5	1/4		+12.95	+9.9	−7.3	−5.3	−17.85
	7.0	1/2		+10.4	+6.75	+3.35	+7.13	−9.41
	10.5	3/4		+6.45	+5.55	+1.65	+1.25	+2.8
12	3.5	1/4				+5.05		−6.3
	7.0	1/2		+22.8		+9.3		+1.65
	10.5	3/4		+9.35	+20.25	+10.15	+6.85	+8.4
11	3.5	1/4						
	7.0	1/2		−9.6	−13.0	−5.15	−15.55	−18.9
	10.5	3/4		−9.7	−13.52			
9	3.5	1/4		+8.3	−13.8	−11.75	−9.45	−23.8
	7.0	1/2		−3.1	−11.05	−5.95	−16.6	−23.075
	10.5	3/4		−13.75	−4.15	−8.75	−7.85	−19.05
7	3.5	1/4		0.0	−3.25	−12.2	−14.6	−29.8
	7.0	1/2		+7.95	+4.45	+2.25	−5.3	−12.3
	10.5	3/4		+5.75	+8.8	+8.75	+2.65	−0.05
Total average	3.5	1/4	+15.387	+7.67	−4.16	+0.84	−14.67	−22.76
	7.0	1/2	+10.75	+6.73	−0.71	+0.92	−6.58	−15.67
	10.5	3/4	+1.37	+0.11	+1.31	+0.075	−2.15	−5.26

Vanessa than continuous light is in harmony with the results obtained in experiments upon the human eye which have been described previously. If McDougall and others are correct in their contention that a given light acting for a given length of time upon the human retina appears brighter than the same light acting for a longer period, it is to be expected that intermittent light of a certain flash-frequency will have a greater stimulating efficiency upon the human eye than continuous light. This expectation is confirmed by the results of the investigations of von Kries and Brücke, who maintain that a disk upon which are painted black and white sectors when rotated at a certain rate appears brighter than when rotated at other more rapid rates. This is also confirmed by the results obtained by Hyde and

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Cady. These investigators contend that the flickering light obtained by rotating certain types of incandescent lamps at a rate of 3 revolutions per second seems to some observers to be brighter than continuous light of the same illumination. Moreover, it is supported by the results obtained by Ferry on the human eye, which have been presented previously.

VI. THE RELATION BETWEEN STIMULATING EFFICIENCY AND THE RATIO BETWEEN THE DURATION OF THE LIGHT AND DARK PERIODS OF INTERMITTENT LIGHT

In the preceding section it was demonstrated that the stimulating efficiency of intermittent light varies with the flash-frequency. In this section it will be demonstrated that it also varies with the ratio between the duration of the flashes and the dark periods between the flashes.

In the experiments described below the butterflies were exposed at the intersection of two beams of light of equal illumination arranged as in the preceding experiments, the light in one beam being continuous and that in the other intermittent. They were given tests with the flash-frequency of the intermittent light 30, 20, 15, 10, 5 and 2 per second and with the ratio between the light and dark periods of the intermittent light 1/3, 1/1, and 3/1. The intermittent light used was produced by means of three different rotating sectored disks, one in which one-fourth of the disk was removed, one in which one-half of the disk was removed, and one in which three-fourths of the disk was removed. In every test made the total illumination of the intermittent light was equal to that of the continuous light. In this way the stimulating efficiency was ascertained of light of various flash-frequencies in which the length of the light and dark periods were in the following ratios respectively, 1 to 3; 1 to 1; and 3 to 1. Fourteen butterflies were tested in all, some under nearly all of the above conditions and others under only a few of them. The results obtained varied considerably in different individuals. In some respects, however, there was but little variation.

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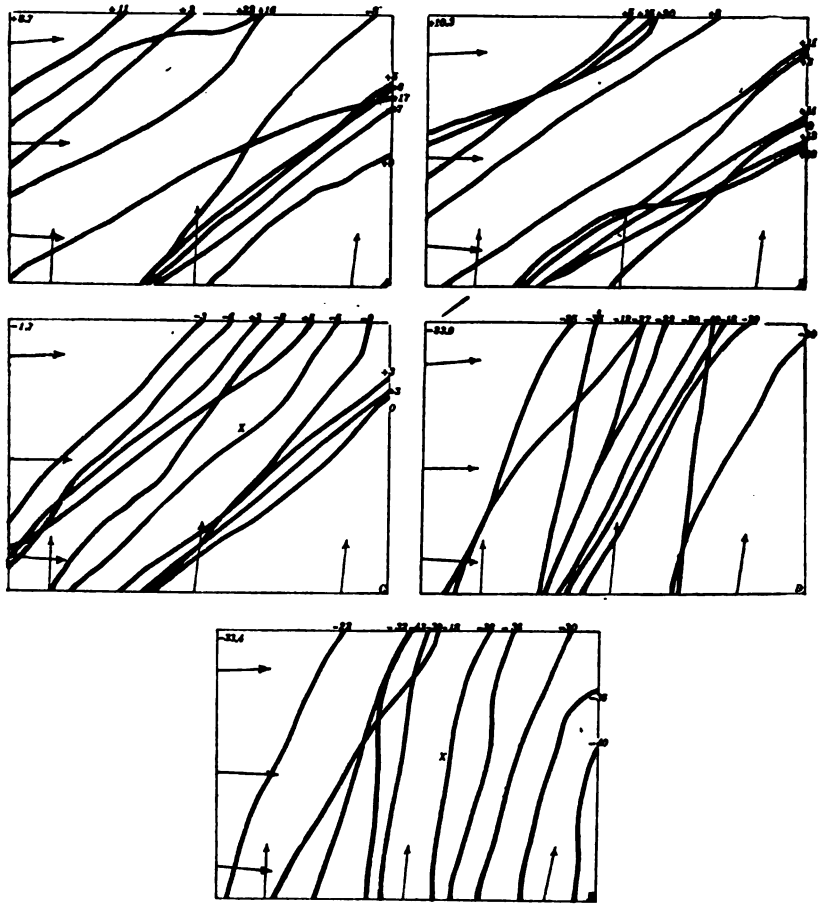

FIG. 2. Paths of a typical specimen of Vanessa in a field of light composed of two horizontal beams of equal illumination crossing at right angles, the light in one beam being continuous and that in the other intermittent. (Reduced by three-fourths). The intermittent light was produced by a rotating sectored disk with one-fourth removed. Arrows to the left, direction of the rays of intermittent light; arrows below, direction of the rays of continuous light. Flash-frequency of intermittent light in *A*, 30; in *B*, 20; in *C*, 10; in *D*, 5; in *E*, 2 per second. Illumination in each beam, 3.5 m.c. Figures above and to right, degree of deflection in individual trials from line bisecting angle between the two beams. "Plus" indicates deflection toward source of intermittent light; "minus," toward source of continuous light. Figures in upper left hand corners indicate the average angle made in each group of 10 trials. The angles given in table 5 for this butterfly (25) are the average of 20 trials. In this figure only 10 of them under the different conditions are presented. Note that the degree of deflection varied with the flash-frequency. When the frequency of interruption was 2 per second the butterfly moved toward a point near the source of continuous light. When the frequency of interruption was 20 and 30 per second the organism moved toward a point some distance from the source of intermittent light. At intermediate flash-frequencies it moved toward points between the above.

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The methods used and the results obtained may be elucidated by a detailed description of one of the numerous similar experiments performed. The animal used in this experiment was designated butterfly "25." This insect was first tested in the flash-frequencies mentioned above with the dark periods of the intermittent light three times as long as the light periods: after an interval of about 30 minutes it was tested with the dark periods of the intermittent light equal to the light periods: finally after another interval of about 30 minutes it was tested with the dark periods only one-third as long as the light periods.

With the ratio between the light and dark periods, 1/3, and with a flash-frequency of 2 per second the average angle of deflection obtained for 20 trials was −23.3 degrees; with a flash-frequency of 5 per second the average angle of deflection was −18.15 degrees; with a flash-frequency of 10 per second the average angle of deflection was +2.45 degrees; with a flash-frequency of 20 per second the average angle of deflection was +12.55 degrees; and with a flash-frequency of 30 per second the average angle of deflection was +11.6 degrees (fig. 2).

With the ratio between the light and dark periods, 1/1, and with a flash-frequency of 2 per second the average angle of deflection obtained for 20 trials was −13.15 degrees; with a flash-frequency of 5 per second the average angle of deflection was −3.05 degrees; with a flash-frequency of 10 per second the average angle of deflection was +1.9 degrees; with a flash-frequency of 20 per second the average angle of deflection was +14.65 degrees; and with a flash-frequency of 30 per second the average angle of deflection was +11.65 degrees (fig. 3).

With the ratio between the light and dark periods, 3/1 and with a flash-frequency of 2 per second the average angle of deflection obtained for 20 trials was −6.95 degrees; with a flash-frequency of 5 per second the average angle of deflection was +5.25 degrees; with a flash-frequency of 10 per second the average angle of deflection was −1.4 degrees; with a flash-frequency of 20 per second the average angle of deflection was +6.75 degrees; and with a flash-frequency of 30 per second the average angle of deflection was +4.95 degrees (fig. 4).

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These results show that the stimulating efficiency of intermittent light depends not only on the flash-frequency but also on the ratio between the duration of the flashes and the intervals between the flashes. This conclusion is evident if the lights

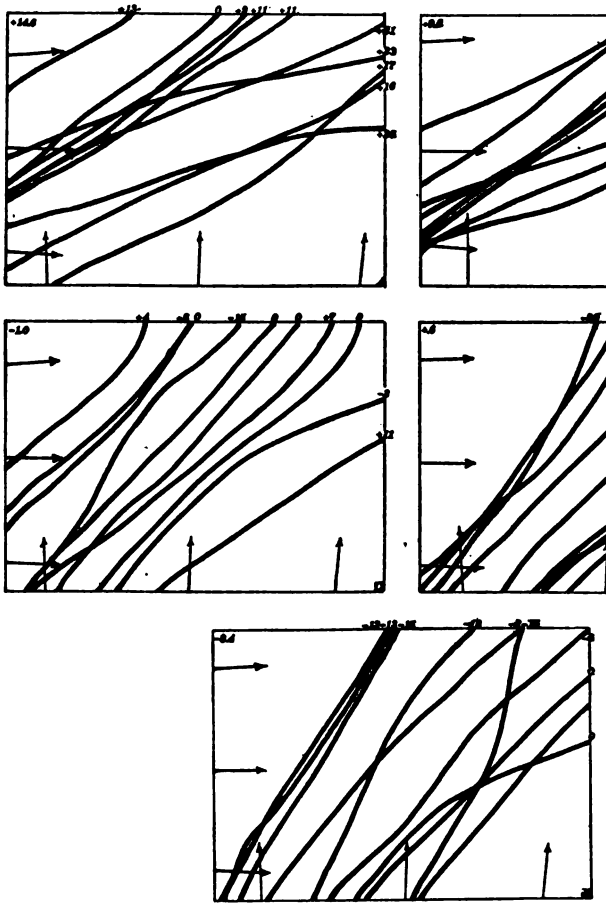

FIG. 3. Paths of a typical specimen of Vanessa in a field of light composed of two horizontal beams of equal illumination, 7 m.c. crossing at right angles, the light in one beam being continuous and that in the other intermittent. (Reduced by three-fourths). The intermittent light was produced by a disk with one-half removed. Other explanations of this figure are the same as those already given in the description of figure 2. Note here also that the degree of deflection varied with the flash-frequency.

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of the same flash-frequency but of different ratios between the light and dark periods are arranged in order as to relative stimulating efficiency. When this is done it is apparent that those lights of flash-frequencies of 30, 20, and 10 per second in which

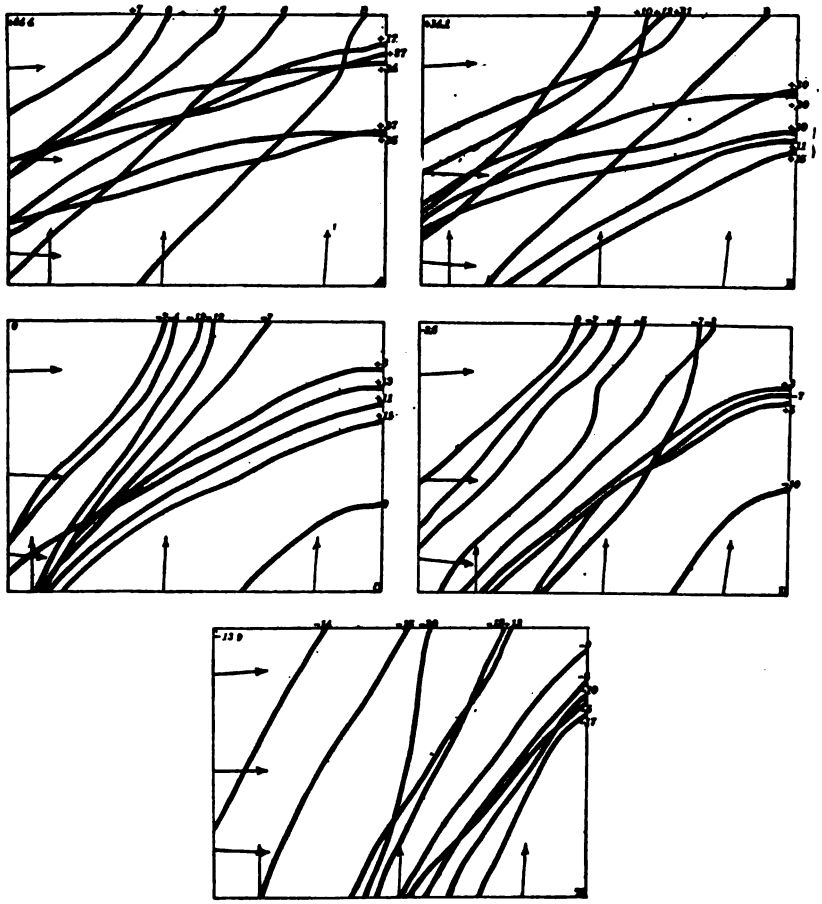

FIG. 4. Paths of a typical specimen of Vanessa in a field of light composed of two horizontal beams of equal illumination, 10.5 m.c., crossing at right angles, the light in one beam being continuous and that in the other intermittent. (Reduced by three-fourths). The intermittent light was produced by a disk with three-fourths removed. Other explanations of this figure are the same as those given in the description of figure 2. Note here also that the degree of deflection varied with the flash-frequency.

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the ratio between the duration of the light and dark periods is 1/3 are of higher stimulating efficiency than the lights of corresponding flash-frequency but in which the ratio between the light and dark periods is 1/1 and 3/1. Similarly, those lights of flash-frequencies of 5 and 2 per second in which the ratio between light and dark periods is 1/3 are of lower stimulating efficiency than those lights of corresponding flash-frequency but in which the ratio between light and dark periods is 1/1 and 3/1. This indicates that the stimulating efficiency of intermittent light depends on the ratio between the duration of the light and dark periods and that the ratio at which it is most efficient at higher flash-frequencies is different from that at which it is most efficient at lower flash-frequencies. This conclusion is confirmed by the results obtained in all of the experiments made which are presented in tables 5 and 6. The significance of the results presented in table 5 is not evident, however, until the data are analyzed, as is done in table 6.

The data in table 6 may be made clearer by a brief explanation of those given for one of the lights, e.g., intermittent light of a flash-frequency of 15 per second produced by a disk with one-half removed. This is given the serial number "2" because 66.66 per cent, or 4 out of the 6 butterflies tested in this light and in the light of a flash-frequency of 15 per second produced by a disk with three-fourths removed deflected more toward the source of intermittent light in the former than in the latter. In the same way a greater proportion of butterflies deflected more in the same direction in the lights listed under serial order number "1" than in those under "2."

It is to be noted that the relative stimulating efficiency of many of the lights is determined by comparatively small percentages. For example, the light of a flash-frequency of 15 produced by a disk with one-fourth removed is assigned a higher stimulating efficiency than the light of a flash-frequency of 2 per second produced by a disk with three-fourths removed because only 60 per cent of the animals tested in both of these lights deflected more toward the source of intermittent light than toward the other source in the former than in the latter light.

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TABLE 6

Relation between stimulating efficiency and the ratio between the duration of the flashes and the dark periods between the flashes, as ascertained by an analysis of the data given in table 5. Lights of the same flash-frequency are arranged in groups. "y," arbitrary unit, amount of light energy emitted in a single flash of intermittent light of flash-frequency of 30 per second produced by rotating disk with one-fourth removed

GROUP	SERIAL ORDER OF STIMULATING EFFICIENCY	PORTION OF SECTORED DISK REMOVED	FLASH-FREQUENCY PER SECOND	RATIO BETWEEN THE DURATION OF THE FLASHES AND THE DARK PERIODS BETWEEN THE FLASHES	AMOUNT OF LIGHT ENERGY EMITTED IN ONE SECOND	PERCENTAGE OF BUTTERFLIES THAT DEFLECTED MORE TOWARD THE SOURCE OF INTERMITTENT LIGHT IN THE LIGHT OF THE PRECEDING THAN IN THAT OF THE SUCCEEDING SERIAL NUMBER
I	1	¼	30	1/3	30 y	
	1	½	30	1/1	120 y	
	4	¾	30	3/1	270 y	66.66
II	1	¼	20	1/3	30 y	60.00
	1	½	20	1/1	120 y	83.33
	4	¾	20	3/1	270 y	66.66
III	2	½	15	1/1	120 y	66.66
	3	¾	15	3/1	270 y	
	9	¼	15	1/3	30 y	60.00
IV	5	¾	10	3/1	270 y	63.63
	6	½	10	1/1	120 y	66.66
	8	¼	10	1/3	.30 y	60.00
V	7	¾	5	3/1	270 y	77.77
	10	½	5	1/1	120 y	91.66
	12	¼	5	1/3	30 y	100.00
VI	10	¾	2	3/1	270 y	90.90
	11	½	2	1/1	120 y	63.63
	13	¼	2	1/3	30 y	

It is probable that tests of larger numbers of butterflies would change the relative stimulating efficiency assigned to several of the lights.

Table 6 shows that the differences in illumination of the various lights used does not cause the differences in stimulating efficiency of these lights, for lights differing greatly in the amount of light

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energy emitted in a unit of time are of equal stimulating efficiency. For example, intermittent light of a flash-frequency of 20 per second produced by a disk with one-fourth removed and light of the same flash-frequency produced by a disk with one-half removed are of equal stimulating efficiency. Yet the latter of these two lights emits four times the amount of light energy in one second that the former does.

Table 6 also shows that the stimulating efficiency of the lights used depends on the ratio between the duration of the light and dark periods. This is evident when the members of each of the groups in this table are studied. For example, in group I are placed those lights of a flash-frequency of 30 per second and in group VI are placed those lights of a flash-frequency of 2 per second. Any difference in stimulating efficiency between the members of each group is therefore not due to differences in flash-frequency for this factor is the same in all. Hence differences in stimulating efficiency between the members of each group must be due to the ratio between the duration of the light and dark periods. In the first two groups the order of the stimulating efficiency of the lights of the three ratios is as follows: 1/3, 1/1, and 3/1, i.e., in each of the first two groups the intermittent light in which the ratio of the duration of the light to the dark periods is 1/3 is of the highest stimulating efficiency, and the intermittent light in which the ratio of the duration of the light to the dark periods is 3/1 is of the lowest stimulating efficiency. Similarly, in the last three groups the order of the stimulating efficiency of the lights of the three ratios is as follows: 3/1, 1/1, and 1/3. This shows that the stimulating efficiency of the intermittent light used in the experiments depends on the ratio between the duration of the light and dark periods and that the effect of the ratio is reversed at lower flash-frequencies.

VII. INFLUENCE OF MECHANICAL STIMULATION AND PREVIOUS EXPERIENCE ON THE REACTIONS OF VANESSA TO LIGHT

When two paths made under the same conditions differ in the angle made with the rays of light it indicates a change in the physiological state of the organism. Two factors were dis-

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covered which may influence the physiological state of the organism and consequently the angle made in a field of light composed of two horizontal beams crossing at right angles. They are present stimulation and previous experience of the organism.

The effect of the first of these was demonstrated in the following manner. A butterfly designated "21" was given 10 trials in two beams of continuous light which crossed at right angles. The illumination in one beam was four times stronger than that in the other. The average angle made in these trials was −9.5 degrees (see fig. 5). After each trial the butterfly was allowed to walk onto a piece of cardboard by means of which it was

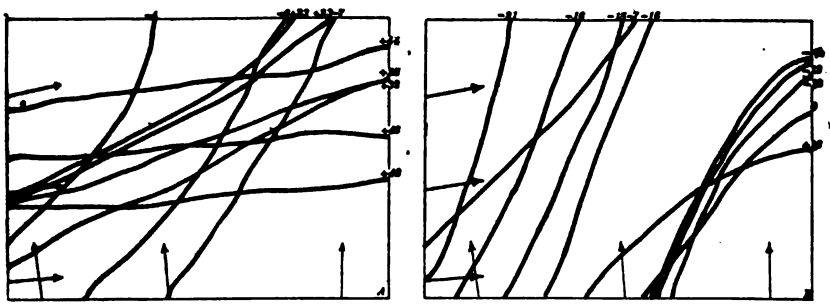

FIG. 5. Paths of a specimen of Vanessa (butterfly 21) showing effect of stimulation on orientation in a field of continuous light composed of two horizontal beams crossing at right angles. (Reduced by three-fourths). Illumination in beam to left, 3.5 m.c.; in beam below, 14 m.c. Figures above and to right, degree of deflection in individual trials from line bisecting angle between the two beams. "Plus" indicates deflection toward weaker light; "minus," toward stronger light. Conditions in *A* and in *B* were identical except that before each of the trials in *B* care was taken not to stimulate the insect, while before each of the trials in *A* the animal was stimulated by being shaken violently in the hollow of the hand. Note that stimulation caused the insect to react more strongly to the weak light and to react very slightly, if at all, to the strong light in 7 out of 10 trials.

carefully transferred to the starting point for a new trial so as to avoid stimulation as much as possible. After completion of these trials the organism was given 10 more trials under the same conditions, except that before each of these trials the insect was stimulated by being picked up in the fingers, placed in the hollow of the hand, and shaken violently. The average angle

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made in these 10 trials was +22.4 degrees. This shows very clearly that mechanical stimulation may greatly affect the deflection in continuous light from two sources. Here "plus" indicates deflection toward weaker light and "minus" toward stronger light. Where used elsewhere in this paper "plus" indicates deflection toward stronger continuous light or toward intermittent light.

Stimulation also may affect the angle made in intermittent light. As is shown in figure 6, butterfly 21 was given 7 trials in intermittent light of a frequency of interruption of 5 per second by the method described previously. Care was taken, as in the tests in continuous light, not to stimulate the animal. The

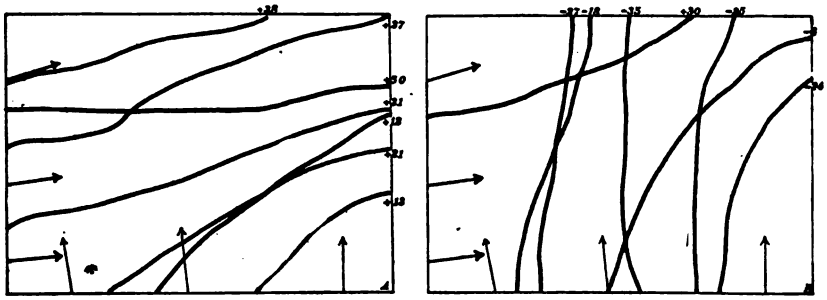

FIG. 6. Paths of a specimen of Vanessa (butterfly 21) showing effect of stimulation on orientation in intermittent light. The field of light is composed of two horizontal beams of equal illumination, 3.5 m.c., crossing at right angles, the light in one beam being continuous and that in the other intermittent. (Reduced by three-fourths). Intermittent light is of frequency of interruption of 5 per second produced by disk with one-fourth removed. Other explanations are the same as those given in the description of figure 2. Conditions in *A* and in *B* were identical except that before each of the trials in *B* care was taken not to stimulate the insect, while before each of the trials in *A* the animal was vigorously stimulated by handling. Note that stimulation caused the insect to react strongly to intermittent light and very slightly, if at all, to continuous light in 6 out of the 7 trials

average angle made in these trials was −16.4 degrees. Immediately afterwards the butterfly was given 7 trials under the same conditions, but before each of these the insect was stimulated by being shaken violently in the hand. The average angle made in these 7 trials was +27.4 degrees. This test shows clearly that stimulation may also greatly affect the angle made with the rays of light if the light in one of the beams is intermittent.

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It is to be noted that in the tests in continuous light (fig. 5) stimulation of the butterfly had the effect of making the organism move almost directly toward the weaker source of light, although the illumination in the other beam was four times greater. Even when the animal began a trial facing the stronger source, it eventually turned toward the weaker source. This persistence in turning toward the same source may possibly be due to the fact that in the first five trials it faced the weaker source and in the succeeding trials, although started in each trial in the other direction, it turned in the same direction as that in which it had turned first. This is evident if previous experience may affect the reactions of this insect, as is shown below.

When the animal started toward the weaker source the nerve impulses aroused by light passed along certain nerves, and during this time the pathways were probably closed to other nerve impulses set up by the stronger light when the insect reached that point where it was exposed to the stronger illumination. If this is true, there is in this case a conflict between stimuli of the same nature, and while the butterfly is reacting to the first it does not react to the second, which may be stronger. This phenomenon is probably similar to that property of higher organisms generally known as "attention."

A second factor which may influence the direction of the course taken in light from two sources is, as stated above, the previous experience of the butterfly. This is shown very beautifully in figure 7. Butterfly 25 was given, in the method described previously, 10 trials in two beams of continuous light, the illumination in one being 4 times that in the other. The average angle made was +9.6 degrees. Immediately after this the organism was given 10 trials in two beams of equal illumination, the light in one beam being continuous and that in the other intermittent of a flash-frequency of 30 per second produced by a disk with one-fourth removed. The average angle made in these trials was +16.7 degrees. Then the insect was given 10 more trials in continuous light under the same conditions as at first. The average angle made in these trials was +21.6 degrees. Why did the animal at one time make an

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average angle of +9.6 degrees and then a little later under the same conditions make an average angle of +21.6 degrees? This difference is probably due to the fact that before the first test the organism had not been tested experimentally at all. Before the second series of trials it had just been tested in intermittent

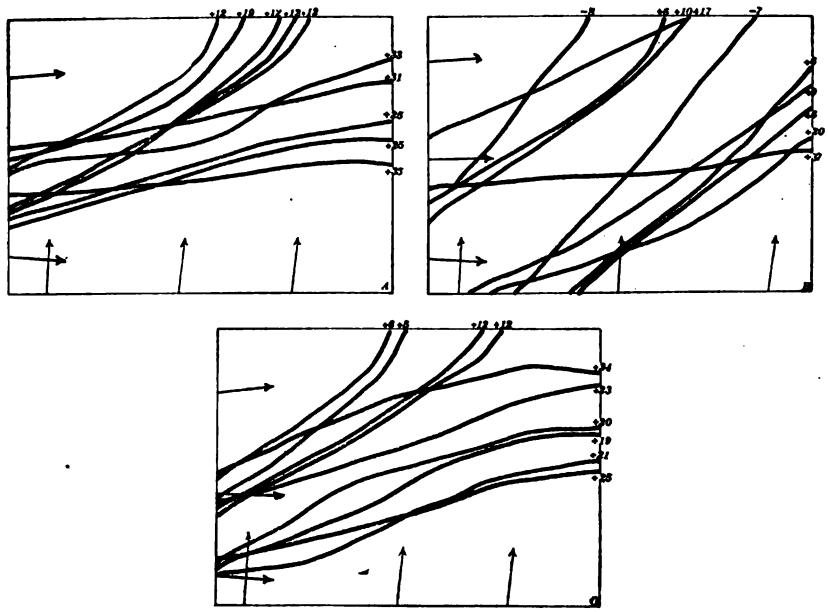

FIG. 7. Paths of a specimen of Vanessa (butterfly 25) showing the effect of previous experience on its reactions in a field of continuous light composed of two horizontal beams crossing at right angles. (Reduced by three-fourths.) Arrows to the left in *A* and in *B*, direction of rays of continuous light of illumination, 14 m.c.; arrows below in *A* and in *B*, direction of rays of continuous light of illumination, 3.5 m.c. Arrows to the left in *C*, direction of rays of intermittent light of flash-frequency of 30 per second produced by disk with one-fourth removed. Arrows below in *C*, direction of rays of continuous light. Illumination in each beam in *C*, 3.5 m.c. Figures have same significance as in preceding figures. In *A* and *B* "plus" indicates deflection toward stronger light; "minus," toward weaker light. In *C*, "plus" indicates deflection toward intermittent light; "minus," toward continuous light. Conditions in *A* and in *B* were exactly identical except that the butterfly had not been tested experimentally before the trials in *B*. It was however given the trials presented in *C* immediately before making the paths reproduced in *A*. Note the difference between the paths in *A* and in *B*. This difference is due to the fact that this organism in *A* was reacting, in part at least, to conditions which had ceased to exist, i.e., to the conditions to which it had just been reacting in *C*.

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light of a flash-frequency of 30 per second, which has been shown to have a great stimulating effect upon this specimen of Vanessa, causing movement toward the source of intermittent light. The butterfly for 10 trials had reacted to this light from this direction, and then when the conditions were changed the

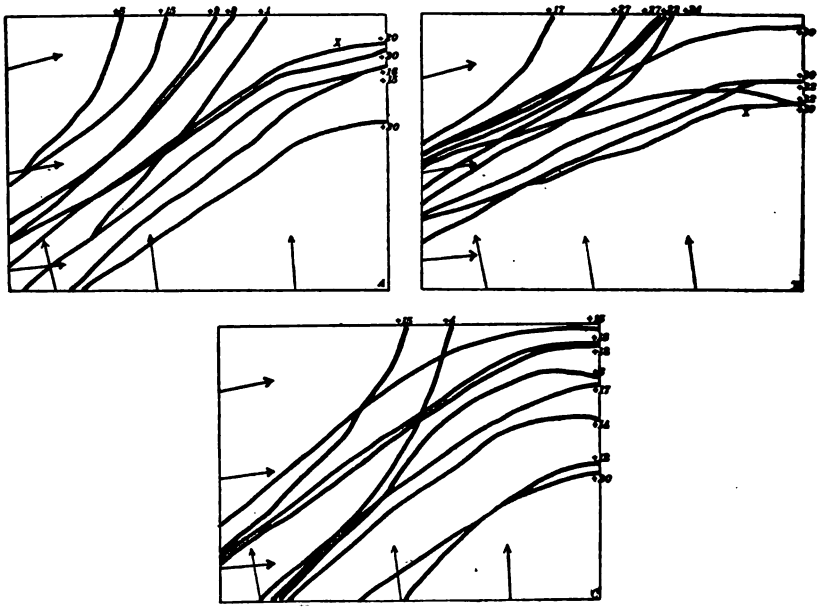

FIG. 8. Paths made by a specimen of Vanessa (butterfly 20) showing the effect of previous experience on its reactions in a field of light composed of two horizontal beams at right angles with each other. (Reduced by three fourths). Arrows to the left in *A* and in *B*, direction of rays of intermittent light. Arrows below in *A* and in *B*, direction of rays of continuous light. Illumination in each beam in *A* and *B*, 10.5 m.c. Arrows to left in *C*, direction of rays of continuous light, illumination, 14 m.c.; arrows below in *C*. direction of rays of continuous light, illumination, 10.5 m.c. Intermittent light of flash-frequency of 2 and 10 per second in *A* and in *B*, respectively, produced by disk with three-fourths removed. Figures with prefixed signs have the same significance as in figure 7. While the animal was making trails marked X in *A* and in *B* the conditions were not the same as in the other trials in *A* and in *B*. During these two trials a beam of continuous light was substituted for the beam of intermittent light, thus changing the conditions to those which were present in *C*. Trails X were tenth in each series. Note that although the conditions were different, about the same angles were made in trails X as in the other trails in *A* and in *B*. Note also that in making trails X the insect was reacting in part to light conditions which had ceased to exist. In *C* are reproduced other trails made by this butterfly under exactly the same light conditions as those in which trails X were made.

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insect continued to behave as it had just been doing. It deflected in the same direction. It was not reacting to the conditions which existed at that moment, but it was reacting to stimuli which had ceased to exist. This is probably an example of organic memory. This conclusion is confirmed by tests on another butterfly which are presented in figure 8.

This insect (fig. 8, *C*) was given 10 trials in two beams of continuous light, the illumination in one being 14 m.c. and that in the other 10.5 m.c. The angles made by the paths ranged from +4 to +20 degrees. It was then given 9 trials in two beams of equal illumination, 10.5 m.c., one of continuous and the other of intermittent light of a flash-frequency of 10 per second produced by a disk with three-fourths removed (fig. 8, *B*). The angles made by these paths ranged from +17 to +39 degrees. Suddenly the light conditions were changed so that they were the same as those in which the previous 10 trials had been made. The tenth trial was then given in these conditions (both beams of continuous light). The angle made was +32 degrees, an angle very nearly like those that had just been made under different light conditions and very unlike the angles that had previously been made under similar conditions. It is probable that in making this tenth trial the insect was reacting to light conditions that had ceased to exist. Its behavior was influenced still by its previous experience. If this is true it must be concluded that the orientation of Vanessa in light from two horizontal beams which cross at right angles depends upon its previous experience.

VIII. SUMMARY

1. When Vanessa antiopa is exposed to continuous light from two sources, the rays of which cross at right angles, it moves toward a point between the sources. The location of this point depends on the relation between the illumination received from the two sources.

2. The stimulating efficiency of intermittent light in the orientation of Vanessa varies with the flash-frequency. At flash-frequencies of 20 and 30 per second it is higher than that

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of continuous light; at flash-frequencies of 5 and 2 per second it is lower than that of continuous light; and at flash-frequencies of 10, 100, 60, 50, and 40 per second it is approximately equal to that of continuous light.

3. The stimulating efficiency of intermittent light of relatively low flash-frequency depends on the ratio between the duration of the light periods and the dark periods. At flash-frequencies of 30 and 20 per second, intermittent light in which the ratio between the light and the dark periods is 3/1 is less efficient than light in which this ratio is 1/3 and 1/1. At these flash-frequencies the efficiency of the latter two sorts of light is apparently equal. At flash-frequencies of 10, 5, and 2 per second intermittent light in which the ratio between light and dark periods is 3/1 is more efficient than light in which this ratio is 1/1, and this light in turn is more efficient than light in which the ratio is 1/3.

4. Some specimens of Vanessa exhibit great variation in their reactions to light. This variation is shown to depend, in part at least, upon previous experience. Mechanical stimulation preceding exposure to light may cause butterflies to react more strongly to weak light than to strong light. It may also cause them to react more strongly to intermittent light of low flash-frequency than to continuous light of equal illumination. Moreover, butterflies after having reacted for some time to certain light conditions may continue to react in the same manner after the light conditions are suddenly changed.

REFERENCES

ABNEY, W. DE W. 1907 Treatise on photography. 10th ed. London, pp. 425.

AUBERT, H. 1864 Physiologie der Netzhaut. Breslau.

BAADER, E. G. 1891 Ueber die Empfindlichkeit des Auges gegen Lichtwechsel. Dissertation, Freiburg, pp. 38.

BASLER, ADOLF 1911 Ueber die Verschmelzung von zwei nacheinander erfolgenden Lichtreizen. Arch. f. d. ges. Physiol., cxliii, 245–251.

BLAAUW, A. H. 1909 Die Perzeption des Lichtes. Rec. des Travaux Botaniques Neerlandais, v, 209; Review in Bot. Cent., 1910, cxiii, 353–356.

BLOCH, A. M. 1885 Experiences sur la vision. Compt. Rend. de la Soc. de Biol., xxxvii.

BRÜCKE, E. 1864 Ueber den Nutzeffect intermittirender Netzhautreizungen. Wiener Berichte, xlix, II, 128.

Digitized by Google

CHARPENTIER, A. 1887 Compt. Rend. de la Soc. de Biol., iv, series 8;
CHARPENTIER, A. 1890 Arch. d'Ophthalmol., x.
CLARK, O. L. 1913 Ueber negativen Phototropismus bei Avena sativa. Zeitschr. f. Botanik, v, 737–770.
DOLLEY, W. L., JR. 1916 Reactions to light in Vanessa antiopa, with special reference to circus movements. Jour. Exp. Zoöl., xx, 357–420.
DOLLEY, W. L., Jr. 1917 The rate of locomotion in Vanessa antiopa in intermittent light and in continuous light of different illuminations, and its bearing on the continuous action theory of orientation. Jour. Exp. Zoöl., xxiii, 507–518.
DUNLAP, KNIGHT 1915 The shortest perceptible time-interval between two flashes of light. The Psychological Review, xxii, 226–250.
EMSMANN, H. 1854 Ueber die Dauer des Lichteindrucks. Pogg. Ann. lxxxxi, 611–618.
EWALD, W. F. 1913 The applicability of the photochemical energy law to light reactions in animals, Science, xxxviii, 236.
EWALD, W. F. 1914 Versuche zur Analyse der Licht- und Farbenreaktionen eines Wirbellosen (Daphnia pulex), Ztschr. f. Psychol. u. Physiol. d. Sinnesorg., xlviii, Abt. 2, 285–324.
EXNER, S. 1868 Ueber die zu einer Gesichtswahrnehmung nöthige Zeit, Sitz.-Ber. d. Wiener Akad. Abth. II, lviii, 601–632.
EXNER, S. 1875 Experimentelle Untersuchung der einfachsten psychischen Processe, Pflüger's Archiv., xi, 403–432.
FERRY, E. S. 1894 The use of the rotating sectored disc in photometry, Phys. Rev., i, 338–345.
FICK, A. E. 1863 Ueber den seitlichen Verlauf der Erregung in der Netzhaut, Reichert's und du Bois Reymond's Archiv., p. 739.
FITTING, H. 1905 Untersuchungen über den geotropischen Reizvorgang. Jahrb. f. wiss. Bot., xli, 221–398.
FRÖSCHEL, P. 1908 Untersuchung über die heliotropische Präsentationszeit. Sitzngsb. Akad. Wiss. Wien. mathem.-naturw. Kl., cxvii. 235–256; 1909, cxviii, 1247–1294.
FRÖSCHEL, P. 1910 Ueber allgemein, im Tier- und Pflanzenreichgeltende Gesetze der Reizphysiologie, xi, 43–64.
HELMHOLTZ, H. L. F., VON 1896 Physiolog. Optik., II Auflage, Hamburg und Leipzig, L. Voss., pp. 1334.
HELMIC, P. S. 1918 The variation in the blackening of a photographic plate with time of exposure, total energy remaining constant. Physical Review, Second Series, xi, 372–375.
HYDE, EDWARD P. 1906 Talbot's law as applied to the rotating sectored disk. Bull. Bureau of Standards, ii, no. 1, 1–32.
HYDE, E. P., AND CADY, F. E. 1906 On the determination of the mean horizontal intensity of incandescent lamps by the rotating lamp method. Bull. Bureau of Standards, ii, no. 3, 415–437.
KLEINER, A. 1874 Zur Theorie der intermittirenden Netzhautreizung. Inaug. Diss. Zürich.
KLEINER, A. 1878 Physiologisch-optische Beobachtungen. Ueber Talbot's Gesetz. Pflüger's Archiv., xviii, 542–573.

Digitized by Google

KRIES, J. v. 1905 Nagel's Handbuch der Physiologie des Menschen, iii, 230–256.
KRIES, J. v. 1907 Ueber die zur Erregung des Sehorgans erforderlichen Energiemengen. Zeitschr. f. Sinnesphysiol., xli, 373–394.
KRON, E. 1913 Ueber das Schwärzungsgesetz photographischer Trockenplatten. Ann. der Phys., xli, 751–758.
KUNKLE, A. J. 1874 Ueber die Abhängigkeit der Farbenempfindung von der Zeit. Pflüger's Archiv., ix, 197.
LOEB, J. 1907 Concerning the theory of tropisms, J. Exp. Zoöl., iv, 151–156.
LOEB, J. 1918 Forced movements, tropisms, and animal conduct. Philadelphia, pp. 209.
LOEB, J., AND EWALD, W. F. 1914 Ueber die Gültigkeit des Bunsen-Roscoeschen Gesetzes für die heliotropische Erscheinung bei Tieren. Centr. Physiol., xxvii, 1165–1168.
LOEB, J., AND NORTHRUP, JOHN H. 1917 Heliotropic animals as photometers on the basis of the Bunsen-Roscoe law for heliotropic reactions. Proceedings of the National Academy of Sciences, iii, no. 9, 539–544.
LOEB, J., AND WASTENAYS, H. 1917 A reexamination of the applicability of the Bunsen-Roscoe law to the phenomena of animal heliotropism. Jour. Exp. Zoöl., xxii, 187–192.
LUMMER, O., U. BRODHUN, E. 1896 Photometrische Untersuchungen. VI. Verwendung des Talbot'schen Gesetzes in der Photometrie. Ztschr. für Instr.-kunde, xvi, 299–307.
MARTIUS, GOTZ 1902 Ueber die Daur der Lichtempfindungen, Beiträge zur Psychologie u. Philosophie, Leipzig, i, Heft 3.
MAST, S. O. 1907 Light reactions in lower organisms. II. Volvox. Jour. Comp. Neur. and Psych., xvii, 99–180.
MAST, S. O. 1910 Reactions to light in marine turbellaria. Carnegie Institute of Washington. Year Book No. 9, pp. 131–133.
MAST, S. O. 1911 Light and the behavior of organisms. New York, pp. 378.
MAST, S. O. 1914 Orientation in Euglena with some remarks on tropisms. Biol. Cent., xxxiv, 641–674.
MAST, S. O. 1915 What are tropisms? Arch. f. Entw. Mech., xli, 251–263.
MAST, S. O. 1916 The process of orientation in the colonial organism Gonium pectorale and a study of the structure and function of the eye spot. Jour. Exp. Zoöl., xx, 1–17.
MCDOUGALL, W. 1904 The variation of the intensity of visual sensation with the duration of the stimulus. The British Journal of Psychology, i, pt. 2, 151–189.
NEWCOMER, H. S. 1919 The relation of the sector opening of the sector photometer to the extinction coefficient. Science, n. s., xliv, 241–243.
PARKER, G. H. 1903 The phototropism of the mourning cloak butterfly. Mark Anniversary Volume, 453–469.
PARKER, G. H., AND PATTEN, B. M. 1912 The physiological effect of intermittent and continuous lights of equal intensities. Am. Jour. Physiology, xxxi, no. 1, 22–29.
PATTEN, B. M. 1914 A quantitative determination of the orienting reaction of the blowfly larva (Calliphora erythrocephala Meigen). J. Exp. Zool., xvii, 213–280.

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PATTEN, B. M. 1915 An analysis of certain photic reactions with reference to the Weber-Fechner law. I. The reactions of the blowfly larva to opposed beams of light. Amer. Jour. Physiol., xxxviii, 313–338.

PFUND, A. H. 1914 On the use of the rotating sector in photometry. The Psychological Review, xxi, no. 2, 116–120.

PLATEAU, J. 1835 Betrachtungen über ein von Hrn. Talbot vorgeschlagenes photometrisches Princip. Pogg. Annalen der Physik und Chemie, xxxv, 457.

SCHATERNIKOFF, M. 1902 Ueber den Einfluss der Adaptation auf die Erscheinung des Flimmerns. Zeitschr. f. Psychol. u. Physiol. d. Sinnesorg., xxix, 241–253.

SCHENCK, FR. 1897 Ueber intermittirende Netzhautreizung. Arch. f. d. ges. Physiol., lxviii, 32–54; lxxvii, 44–52.

SCHENCK, FR., UND JUST, W. 1900 Ueber intermittirende Netzhautreizung. Arch. f. d. ges. Physiol., lxxxii, 192–198.

SWAN, W. 1849 On the gradual production of luminous impressions on the eye and other phenomena of vision. Trans. Roy. Soc. Edin.

TALBOT, W. H. F. 1834 Experiments on light; on photometry. Phil. Mag. Series 3, v, 327.

WEYER, E. M. 1899 Die Zeitschwellen gleichartiger und disparater Sinnes Eindrucke, Philos. Studien, xv, 67–138.

WIEDEMANN, E., U. MESSERSCHMITT, J. B. 1888 Ueber Fluorescenz und Phosphorescenz. Gültigkeit des Talbot'schen Gesetzes. Wied. Annalen, xxxiv, 463–469.

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THE RELATION OF PHOTOTROPISM TO SWARMING IN THE HONEY BEE, APIS MELLIFERA L.[1]

DWIGHT E. MINNICH

The honey bee is remarkable for the extent to which many of its activities are controlled by light. Observations and experiments demonstrating the strong photopositive responses of this animal have been detailed by Lubbock ('82, p. 278, 279, 284), Graber ('84) and Hess ('13a, '13b, '17). And indeed this feature of behavior must have been patent to many of the earlier workers, so strikingly and constantly is it exhibited.

While conducting some experiments on the photic behavior of bees several years ago, I was greatly impressed by the strong, positive reaction to light which normal individuals almost invariably displayed. Bees in an active state of locomotion, or easily excited to such, exhibited an orientation which, for its rapidity of accomplishment and its accuracy of maintenance, was most spectacular. I say bees exhibiting vigorous locomotion, for, obviously, bees which are torpid and do not move freely—a condition frequently encountered during cool, damp weather—cannot show phototropism. Even the moribund condition seemed often to intensify rather than to weaken photic behavior, and bees scarcely able to creep were observed making a final struggle toward the light.

My experiments were carried on well into the autumn, when it became more and more difficult to obtain bees in the field as the flowers became less abundant. I, therefore, installed a single comb of worker bees without queen, in a glass observation hive. The hive was kept darkened by means of small wooden covers fitted to the glass sides, and the exit was covered with a bit of screen wire to prevent the escape of the bees. Animals

[1] Contributions from the Zoölogical Laboratory of the Museum of Comparative Zoölogy at Harvard College. No. 321.

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could be kept in this way in reasonably good condition for a month to six weeks, food being provided in case there was not sufficient in the comb. It was on these bees that I made the observations to be detailed in the present note, which I believe afford some evidence on the question of swarming and its relation to light.

The strong positive phototropism of individual bees is also characteristic of certain activities of the hive as a whole. Kellogg ('03) was the first to call attention to the fact that swarming bees are strongly positive to light. He says (p. 694), "That the issuance from the hive at swarming time depends upon a sudden extra development of positive heliotropism seems obvious. The ecstasy comes and the bees crowd for the one spot of light in the normal hive, namely, the entrance opening. But when the covering jacket[2] is lifted and the light comes strongly in from above—my hive was under a skylight—they rush toward the top, that is toward the light. Jacket on and light shut off from above, down they rush; jacket off and light stronger from above than below and they respond like iron filings in front of an electro-magnet which has its current suddenly turned on." These observations leave no doubt as to the strong positive phototropism of swarming bees. The statement, however, that the issuance of the swarm is dependent upon a "*sudden extra development*" of positive phototropism has met with some difference of opinion.

Thus von Buttel-Reepen has objected to this interpretation on the ground of a particular swarm instinct. He says ('15, p. 168)". . . . so ist in diesen Momenten [the emergence from the hive at swarming] nicht ein besonders starker Heliotropismus die Ursache, sondern die Schwärminstinkte drängen die Bienen in das Freie." Another objection, and I believe a much more important one, is suggested by a statement of Hess ('13a, p. 663). "Meine Versuche zeigen, dass die Neigung der Bienen, zum Hellen zu gehen, nicht auf die Zeit des Hochzeitsfluges beschränkt ist." This contention of Hess—that the

[2] Kellogg had a glass observation hive covered with a black cloth jacket.

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manifestation of photopositiveness is not confined to any given period, such as the nuptial flight, has been amply confirmed in my observations. I have already called attention to the fact that among individual bees an active animal exhibits, almost without exception, strong positive phototropism. Large aggregates of bees also show marked positive light reactions quite aside from swarm activities, as the following observations clearly show.

The glass observation hive with its single comb of bees was situated at a south window where it was exposed to the sun during the morning hours. With the oncoming of the cool days of late October and early November, the bees became much less active. On bright days, however, the exposure to the warmth of the sun, and the corresponding rise of temperature within the hive, served to activate the colony until most of the bees were moving about the walls of the hive in a rapid uneasy manner. If at such a time the cover was removed from the end nearer the window, thus admitting the light, there was with the first penetrating rays, a sudden increase of the hum within. Thereupon ensued a scene no less spectacular than that described by Kellogg ('03, p. 693). His words picture it exactly, ". . . . the whole community of excited bees flowed—that is the word for it, so perfectly aligned and so evenly moving were all the individuals of the bee current—," toward the illuminated side. A few hours later, however, when the sun had passed from the hive and the temperature had fallen, particularly on the cooler days, the uncovering of one end of the hive elicited no such response. With perhaps the exception of a very few individuals, the bees remained quietly in the swarm cluster. Nor was this failure to obtain a response a result of the lowered intensity of light, for the active worker bee responds to a fairly low intensity. It must, therefore, have been due to the difference in temperature.

These facts, together with those previously stated, demonstrate conclusively that the pronounced phototropism, so conspicuous in swarming bees, is not confined to the period of swarming. This condition of photosensitivity appears to remain fairly constant at all times in the animal, although active locomo-

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tion and the absence of other strong, distracting stimuli are necessary to its demonstration. The situation here differs from that described by Loeb ('90) for winged male and female ants, in which positive phototropism is confined exclusively to the period of the nuptial flight.

We may agree with Kellogg, therefore, that the issuance of bees from the hive at swarming may be a simple exhibition of positive phototropism, but it is not the result of a sudden increase in phototropism. The sudden increase is in the activity of the animals. Consequently, the fundamental factors in swarm behavior are those which effect a condition of heightened activity, a condition in which locomotion is generally controlled by light. In my own observations the state of heightened activity doubtless arose from the conditions of temperature and the prevention of the bees from making their accustomed flights for defecation, etc. The factors which activate the swarm are not known. They may consist of particular swarm instincts, as von Buttel-Reepen ('15) has suggested, or they may even be more simple reflexes of as yet unknown nature. In any case, the evidence seems clear that, although phototropism may be an important feature of swarm behavior, it is neither peculiar to this activity nor the primary causal agent of it.

REFERENCES

Buttel-Reepen, H. von 1915 Leben und Wesen der Bienen. Braunschweig, 8vo, xiv + 300 pp.

Graber, V. 1884 Grundlinien zur Erforschung des Helligkeits-und Farbensinnes der Tiere. Prag und Leipzig, 8vo, viii + 322 pp.

Hess, C. 1913 a Gesichtssinn. Handb. vergl. Physiol. von Hans Winterstein, iv, 555-840.

Hess, C. 1913 b Experimentelle Untersuchungen über den angeblichen Farbensinn der Bienen. Zool. Jahrb., xxxiv, Abt. allg. Zool. u. Physiol. der Tiere, 81-106.

Hess, C. 1917 New experiments on the light reactions of plants and animals. Jour. Animal Behavior, vii, 1–10.

Kellogg, V. L. 1903 Some insect reflexes. Science, N. S., xviii, 693-696.

Loeb, J. 1890 Der Heliotropismus der Thiere und seine Uebereinstimmung mit dem Heliotropismus der Pflanzen. Würzburg, 8vo, 118 pp.

Lubbock, J. 1882 Ants, bees and wasps. New York, 12mo, xix + 448 pp.

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THE NEED FOR ANALYTICAL STUDY OF THE MAZE PROBLEM

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The maze method of studying the process of learning, as it is so commonly used in animal psychology and to some extent in human, is still employed almost entirely in an unanalyzed manner so far as concerns the apparatus. The particular pattern of labyrinth to be used by an experimenter seems to have been principally determined by the patterns used by earlier workers. The Hampton Court maze is the classic instance. A variation from earlier forms may be forced by such considerations as: the limitations of the learning capacity of the animal to be used; the limitations of available space; the nature of the problem, e.g., whether motor or sensory habits; the limitations of the materials to be used in the construction; etc. Watson and his students have introduced the use of a circular maze, but its particular advantages have not been definitely set forth, so far as the writer is aware. A thorough-going and detailed planning of what may be called the architecture of the mazes that are to be used in a given research is at present impossible on account of our ignorance of the relative values of different elements within a maze problem.

Many analyses have been made of sensory cues as they control the maze running, and some analyses have been made of the data available in scoring runs, but no definite analytical study has been given the maze pattern itself. This would have some importance in the study of the building up of single habits. For instance, there is at present disagreement as to whether an animal tends to eliminate the earlier or the later blind alleys first. This is not to be settled by having two different investigators at different laboratories use two different kinds of mazes.

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Many kinds of mazes must be used, and attempts be made to make the earlier and the later blind alleys as similar as possible in local design. But maze analysis would have perhaps even greater importance in the precise study of the formation of multiple habits and their interrelations. At present it is possible that maze problems are accepted as equal in difficulty that are really not equal, and *vice versa;* again, particular open runways and particular blind alleys are by no means definitely evaluated.

As a sample of how this kind of work can be approached the writer reports a brief study made in the Oberlin College labo-

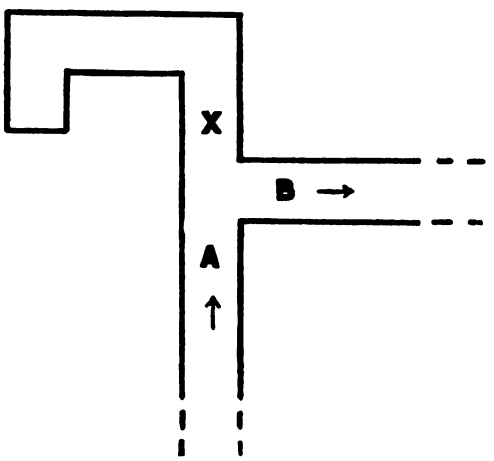

FIG. 1

ratory. His class in animal psychology were reading Watson's "Behavior" for one of their texts, and upon coming to a certain exposition therein that excited their doubt they decided to "try it out" experimentally. In building up his argument for frequency as the most potent factor in the selection and fixing of successful variations from the whole repertoire of an animal's reactions, the author makes use of the following exposition:

Let *A* and *B* (fig. 1) represent the segments of the true pathway and *X* the entrance to any *cul-de-sac* (let the segments be chosen somewhere in the middle of the maze). We will suppose that the animal is on its way to the position *A* for the first time. The chances of entering *B*

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and *X* are equal in the long run when the animal is in the position *A*. If the animal goes into *B* the true pathway scores one in frequency as over against the *cul-de-sac*. If the animal goes into the *cul-de-sac X* and follows it out to the bitter end it must return. When it reaches the position of the letter *X* on the return again the chances of entering *B* and *A* are equal. We thus see that there is a greater probability of the animal's remaining on the true pathway than of his leaving it. Possibly the case can be more definitely presented if we ask for the probability that the animal takes the wrong path. In order that this may occur it must (1) choose the wrong path from *A* to *X* and (2) choose the wrong path from *X* to *A*. The probability of each wrong choice is $\frac{1}{2}$; the probability that both wrong choices be made is $\frac{1}{2} \times \frac{1}{2} = \frac{1}{4}$. The complementary probability or the probability that it takes the right path is $\frac{3}{4}$; i.e., the chances are three to one that the animal goes out through *B* rather than comes back to the starting point. This factor (frequency) alone is probably sufficient to account for the formation of the maze habit (pp. 266–268).

Now, doubt as to some of the quantitative relations assumed arose with the readers. We were not concerned with Watson's underlying thesis but with some details of the presentation. Is the probability of an animal's passing into a straight-ahead pathway the same as that of its turning into one at the side? Again, upon its exit from a blind alley is it as likely to re-enter the old path as to enter upon the new one? Finally, does the position of this blind alley with reference to the true path affect the direction taken upon exit from it?

To get some definite data upon these points, as little complicated with other data as possible, the class adopted the following procedure. With a multiple unit or "take down" system of construction[1] ten maze patterns were arranged, as simple as possible, each pattern containing the essentials of the problem, namely, a blind alley on the right and one on the left of the true path and a blind alley straight ahead or forward (*R*, *L*, and *F*, respectively). They are shown in figure 2.

[1] For description, see R. H. Stetson and J. F. Dashiell. A Multiple Unit System of Maze Construction. Psychological Bulletin, July, 1919, *16*, 223–230.

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Fifty white rats were used as subjects, five being run in each maze for one trial only. The conditions of the problem forbade the use in any trial of a rat that had had any acquaintance with a maze before. The pattern of each maze was reproduced on paper and a pencil line was used to record accurately the particular directions and turns in the rats' trials. The graphic records

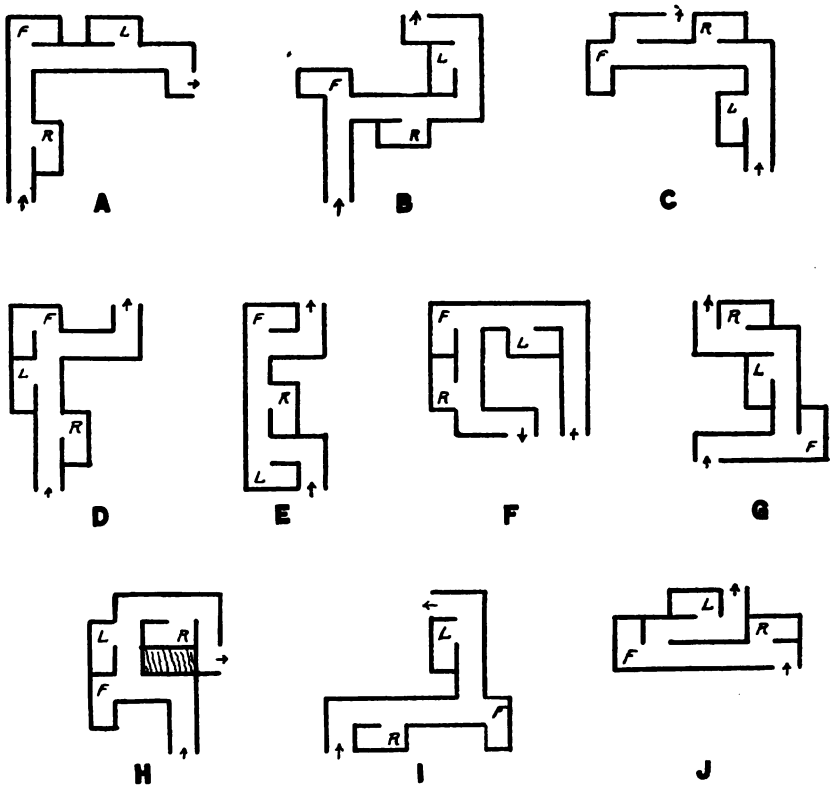

FIG. 2

were then analyzed, by inspecting them and tabulating for each maze and rat the number of times *F*, *R*, and *L* each was passed or was entered, and in the latter cases the number of exits from the blind alley that turned backward (i.e., retracing pathway toward entrance) and the number of exits turning forward (following along the newer path toward the exit from the

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maze). It is to be noted that when an animal was moving backward in the path (i.e., toward the entrance) a turn into a blind alley and out again toward the maze entrance was counted as "forward," and under the same conditions a turn out of the blind alley toward the maze exit was called "backward." Table 1 gives the totals for all mazes and subjects.

TABLE 1

BLIND ALLEYS	PASSED	IN	EXIT BACK	EXIT FORWARD
F	31	53	21	32
R	41	46	10	36
L	38	35	8	27
Average of R and L.....	39.5	40.5	9	31.5

From this table the conclusions, so far as our experiments go, are as follows.

1. A blind alley opening straight ahead is more likely to be entered than to be passed, in about the ratio of 5 to 3.

2. A blind alley opening at the side is about as likely to be entered as to be passed.

3. The exit from a blind alley opening straight ahead is more likely to be in the forward than in the reverse or backward direction, in about the ratio of 3 to 2.

4. The exit from a blind alley opening at the side is more likely to be in the forward than in the reverse direction, in about the ratio of 3½ to 1.

So much for the empirical findings. But theoretical anticipation had not been different, and this because of two considerations.

1. The white rat has been shown to possess a rather reliable sense of direction,[2] and this might be expected to give prepotency to the openings appearing in the forward-going direction. In this case the direction-sense would seem to operate especially as a sense-of-direction-in-which-one-is-going rather than a sense-of-direction-of-destination.

[2] J. F. Dashiell. Some Transfer Factors in Maze Learning by the White Rat, to appear in JOURNAL OF PSYCHOBIOLOGY.

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2. The white rat shows curiosity to a striking degree. Its large supply of energy is readily enlisted in the service of this seeking of the novel. Even when hungry and feeding or when nursing a litter the rat will leave off to examine any new object put into its nest. Other things being equal, this curiosity motive would be expected to re-inforce somewhat the tendency to enter a new runway.

Many other questions in the analysis of the maze problem will suggest themselves to the experimenter, questions concerning the values of different parts of the maze not only for initial trials but also for the whole period of learning. A few of them are:

1. What is the potency of a blind alley opening on a side of the runway and just preceding a turn in the true path in the same direction, as compared with a blind alley just following such a turn or one on the other side of the runway from a turn, etc.?

2. Will a given number of blind alleys offer more difficulties to the learning subject if placed simultaneously in the maze or serially?

3. Does the difficulty of a maze as a whole increase progressively with the number of blind alleys offered? If so, what type of mathematical progression obtains?

4. Is there any change in relative difficulty of different parts of a maze as the subject's trials increase in number?

5. To what extent would the findings on these or other similar questions be dependent upon the particular species and age of subject used?

As hinted before, this report is made not so much on the strength of the numerical findings given as for the outlining of a complex of quantitative analytic problems important to maze work and for whatever of value it suggests in the matter of experimental approach.

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EFFECTS OF ALCOHOL ON HAND AND EYE COÖRDINATION

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In work at various times during the last five years, I have noticed certain effects of alcohol on hand and eye coördination, which resemble the effects of oxygen insufficiency. In some cases where the alcoholized reactor shows little mental effects, and little effect on most motor coördinations, there is nevertheless a noticeable deterioration in voluntary movements of the hand and fingers where both speed and accuracy are required, as in marking charts, drawing simple figures, or in writing. Great individual differences are also found in this respect. In order to make further tests on this point, and to obtain methods which might possibly be applied to the study of causes of inefficiency other than alcoholic, the test sheet reproduced in figure 1 was made up.

In this sheet, nine pathways are provided across the page, each pathway having 20 turns at right angles. The pathways are approximately $\frac{1}{8}$ inch wide, and the distance straight across from the beginning of each to the end is a trifle over 5 inches. Certain confusion lines are added to demand strict visual attention. The beginning of each pathway is marked by an "x."

In the test, the reactor is required to trace each pathway with a pencil, starting from the cross at one end, and not touching any line of the sheet. To provide uniformity, a metronome is used, and the reactor required to trace each straight portion of the pathway with one beat of the metronome. Tracing a single pathway, from left to right requires therefore twenty-one beats of the metronome. By adjusting the rate of the metronome, the proper degree of difficulty can be secured. Each stroke of the pencil is approximately $\frac{5}{8}$ inch long, and the strokes required

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are alternately vertical and horizontal. Some preliminary practice is required, and the positions of the sheet and the hands must be standardized.

Tests with interesting results have been made on two subjects, and are briefly reported here. In these tests, the metronome rate was one beat per second, and the reactor was required to complete the whole page before stopping. If no time had been

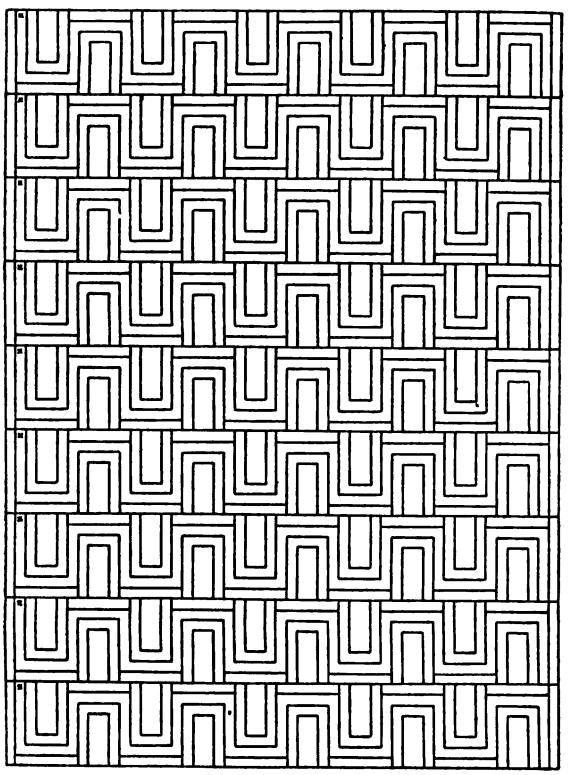

FIG. 1. COÖRDINATION TEST SHEET, REDUCED TO ONE-HALF DIAMETER

Measurements of sheet, five and seven eighths by eight inches. Pathways one-eighth inch wide (approximately). In new forms modified by Buford Johnson for use with children, the vertical strokes are replaced by slanting ones, and the pathways made wider. The sheet here shown was used in the position given, but it has since been found more satisfactory to turn it so that the x's are at the right, and trace from right to left. The new forms have an x at each end of the pathway.

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lost at the ends of the lines, the sheet would have been completed in 189 seconds: but actually from four to nine seconds longer were required in most cases, although reactor B gained a few seconds per pathway towards the end of his tests, by getting out of "step."

Each of the reactors began a fresh sheet approximately every ten minutes, and nine sheets were completed by each. The duration of the whole test for each reactor was therefore one hour and twenty minutes. During this period each consumed 7 ounces of rye whiskey of excellent quality, in four doses of 1½, 2, 2, and 1½ ounces each, each dose being followed by a "chaser" of an equal quantity of water. The first dose was given immediately after finishing the first sheet: the others at 12, 36, and 47 minutes thereafter.

Reactor A showed no appreciable deterioration at any stage of the experiment, but improvement rather. There were only 20 touches in the final sheet as against 25 in the preliminary one. This reactor kept the beat of the metronome throughout, losing time only in moving from the end of one line to the beginning of the next. Reactor B showed significant deterioration, which finally became profound. There were 45 touches in the final sheet as against 13 in the preliminary. This reactor also lost the rhythm toward the end of the period, and gained a few seconds in each sheet.

In each case, an assistant kept the time record, while I observed the reactor carefully. The tracings were made with a pencil instead of a pen, because it was feared that under the influence of alcohol the reactor might have difficulty with a pen. The pencil tracings however do not photograph well, so none of the tracings can be presented here. In counting the errors (touches on the bounding lines) great care was observed not to count an error, however close the tracing to the line, if it did not actually touch.

All the records were made in the usual writing way: i.e., with the fingers supported on the writing surface. The better way in general is to have no support at all except the pencil. It was feared however that the alcoholized patient would begin

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to rest the hand in spite of instructions to the contrary, hence he was allowed to support it on the paper from the start. The tracings were from left to right, as in normal writing. It has since been found better to turn the sheet upside-down and work from right to left.

Comparison of the records with the personality and behavior of there actors is instructive. Reactor A is past thirty years of age, not a regular drinker, but has taken drinks socially at long intervals during the past ten years. Has never been intoxicated. Brilliant intellectually, physically vigorous and athletic. Blood pressure unusually low. Showed no symptoms of intoxication during the test, except a flushing of the face. No ill effects until eight hours afterwards (evening). Then dizziness, weakness, and nausea with vomiting. Felt better after relieving stomach, and showed no effects when seen the next morning.

Rector B is past twenty-five; had seldom tasted liquor or other alcoholic beverages. Keen intelligence, but emotionally unstable, in the sense of being easily depressed or elated. Tends to be excitable at times; has plenty of grit and persistence, physically and intellectually; has slight hyperthyroidism, but otherwise physically sound. Stands exercise (walking, tennis, dancing) well. Has been described as "a good sport." Blood-pressure normal, but pulse rapid. This reactor showed evidence of intoxication at an early phase of the test, and at the end was hysterically drunk; laughed a great deal and staggered violently in attempting to walk. In the test preceding the final one, worked with the chart upside down without noticing it. About an hour after finishing became sick and vomited. Felt better later, and ate some lunch. Sick again in the evening, passed miserable night, and had characteristic sick feeling and headache in the morning. Strong revulsion against alcohol, and usual "never again" attitude, which lasted five days.

The individual differences are strong here, and emphasize the danger of conclusions from experiments on alcohol which neglect the correlation with the physiological and psychological characteristics of the reactors. The necessity of prolonged observations on each subject is also indicated. Subject A very

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probably would have showed coördination characteristics very different from those obtained, if he had been tested in the evening. Unfortunately, the reactors were not tested after the "final" sheet, and their reports next day are the basis of the description of the effects of the twenty hours following the tests. The relative futility of tests of single functions, even if several are tested in succession, is illustrated by reactor B. Although marked deterioration is shown in the coördination, it is not a sufficient index of the degree of intoxication. Yet this test, as our previous work shows, catches the deterioration more definitely than most mental or psychophysical tests.

The test-sheet itself proves to be a good one, and it can very usefully be applied to other problems. Slightly different forms are being prepared for use with children.

NOTE: Since the above was written, I have discovered in a recent monograph on tobacco effects, a somewhat similar form, ascribed to Columbia, and have just obtained a sample of this test. The Columbia form has five pathways, each $\frac{3}{32}$ inch wide, with twenty-seven rectangular turns, with a total distance from right to left of $5\frac{1}{2}$ inches. Only the two lines enclosing the pathway are printed. I believe the additional lines on my sheet are an advantage.

The form I have designed is of course a result of working with Whipple's star figure, Franz' Maze tests, and the Porteus tests.

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PSYCHOLOGICAL EFFECTS OF DEPRIVATION OF OXYGEN—DETERIORATION OF PERFORMANCE AS INDICATED BY A NEW SUBSTITUTION-TEST[1]

H. M. JOHNSON AND FRANKLIN C. PASCHAL

From the Air Service Medical Research Laboratory, Mitchel Field, Long Island, New York. Publication authorized by the Surgeon General, November 4, 1919

INTRODUCTION

In the routine classification-tests of American military aviators according to their ability to resist depletion of their oxygen-supply, the rating of their psychological performance is based on criteria which must be evaluated by the psychological observer during the progress of the test. In this respect the criteria of classification are in a measure "subjective" in character. Like all clinical data, they depend to some extent on the personal biases and different degrees of impressionability of the observers.

A statistical investigation is now in progress at the Air Service Medical Research Laboratory, for the purpose of estimating the importance of the personal equation of the psychologist in his weighing of the criteria. The results obtained to date indicate a definite influence, which usually is not seriously large, and is probably capable of being corrected for.

From the earliest stages of the development of the psychological studies of the effects of deprivation of oxygen, it was recognized that a test which would furnish an "objective" measure of the extent of deterioration of behavior would have greater authority than the "clinical" methods which were finally adopted. One difficulty underlying the development of "objective" tests lies in the fact that the efforts of the subject to compensate for the deterioration in his responses by the expenditure of greater energy in "voluntary" control, are not readily recorded in an interpretable manner. These data are fully as important

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as the variations in speed and accuracy, and in some cases are even more important. Unless they are included in the record, or are at least considered, a mere inspection of the subject's record of performance may give a very misleading impression.

PURPOSE OF THE PRESENT EXPERIMENT

In its essential features, the test described below is one of several which were attempted early in 1918 and abandoned. In January, 1919, the original method was materially improved and used for another purpose than the original one: namely, as a part of a somewhat general attempt to ascertain whether the more complex processes of behavior tend to deteriorate before, after, or simultaneously with, the simpler processes.

A definite impression regarding this question was made on the pioneer investigators, by a mass of observational material yielded by themselves as well as by other subjects.[1] The material as presented in fragmentary form is not readily susceptible of analysis, and the conclusions drawn from it have not been unanimously accepted.

The present type of experiment was selected for use in a part of this investigation, since it involved sustentation of the visual function, attention, memory, and muscular coördination during a longer period than can be included in an ordinary voluntary "spurt." Certain of the factors involved are also capable of being isolated for further study by other methods.

THE METHOD EMPLOYED

The task assigned the subject is the transliteration of a number of sets of nonsense-material into corresponding codes, one of which is presented with each set of material. The codes are from a group compiled by Capt. Johnson with a view of insuring the closest practicable approximation to equality of difficulty. After a large number of systems for deriving them had been tried and rejected, a method of satisfying this condition was found. The

[1] Cf. Dunlap, Knight: Psychological research in aviation. Science, N. S., vol. 49, 1919, pp. 94–97.

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different sets of test-material were derived by transliterating a single set successively into the several codes. Thus the several sets are also of approximately equal difficulty. The codes and the test-material are reproduced in the appendix, with a brief description of the method of preparation.

Most of the tests were administered by Dr. Paschal according to the following method: The subject is seated in front of the Henderson rebreathing apparatus, behind an adjustable desk, to which a large ruled card is secured, below a rack provided for reception of the test-card. (Cf. appendix.)

A sample test-card is placed in the rack, and the subject is instructed to print[2] on the large ruled card the letters presented in the given code to be substituted for the letters in the set of test-material.

The subject is informed that his performance depends on both time and accuracy, and is measured by *the number of letters correctly substituted in a unit of time.*

The subject is then connected with the rebreather, and the work is started.[3]

In the actual experiment, the following procedure is observed with respect to presentation of the material: the test-card is

[2] It was observed during the exploratory work early in 1918 that handwriting became more illegible than printing at advanced stages of oxygen-depletion—a fact which was later made the subject of a special study by Major John B. Watson. In the opinion of Major Dunlap, who was in charge of the earlier work, printing as a substitute for writing also tended to lessen the disadvantage of the more clumsy and excitable subjects.

[3] In the earlier experiments the subjects worked up three sets of material before the mouthpiece was inserted, and two additional sets after the mouthpiece was inserted, and before rebreathing was commenced. In these experiments the unmodified Henderson rebreather was used, and the duration of the experiments was in many cases shorter than was desired. Through a method devised by Capt. D. C. Rogers, of the section of psychology, it became possible to increase the duration of the run as much as was desired, and to control, within reasonable limits, the rate of consumption of oxygen. In these experiments the mouthpiece was inserted and rebreathing was started before the first set of material was presented; but during the first thirteen minutes all the oxygen consumed by the subject was replaced as consumed, so that during that time the air was normal with respect to oxygen-content. The position of the asterisk on the graph and the accompanying note will indicate which type of rebreathing was used in each case.

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laid in the rack, face down. The subject is warned by the spoken words, "Ready! Now!" and the face of the card is exposed. A stop-watch is started when the subject begins to print the first substituted letter, and is stopped when the subject taps on the desk to indicate that the last substituted letter has been printed. To guard against the appearance of certain spurious effects the several sets of test material are given in an irregular sequence, which is different for most of the subjects.

RESULTS OF THE TEST

Unsuccessful tests were run on nine subjects before the method was sufficiently standardized for use. Due to failure of proper control of the apparatus, three other experiments were considered unsatisfactory. These records are not included among the results presented below.

The abscissal values of the curves of psychological performance represent the middle point of the temporal period of work on the given set of material. At first, attempts were made to maintain a constant temporal interval, or "headway," between the presentations of the several sets, but the fluctuations of performance rendered this impracticable. Later, an attempt was made to maintain a constant period of rest between periods of work, and this attempt was more nearly though not completely successful, the principal source of variability being the necessary activity of the clinician.

The scale of ordinates was arbitrarily chosen from the standpoint of convenience in plotting. Each ordinate represents five times the average number of letters correctly substituted per minute.

DISCUSSION OF GRAPHS

Most of the records show an important degree of improvement with practice, as compared with the initial performance. The rate of improvement varies widely with the different subjects, and depends in large degree on their relative composure, distractibility, and interest. When the mouthpiece is first inserted

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(as indicated by an asterisk on the graphs) a noticeable disturbance usually results, but this is usually quickly overcome.

In those cases in which the rate of improvement is rapid, the curve of performance reaches a level rather quickly; and such subjects usually show a definite deterioration when the oxygen-content is depleted to a point between 15 and 12 per cent. In many subjects, this disturbance is compensated for by an increase of muscular activity. The compensation may be partial, as in no. 6877; complete, as in no. 6864; or excessive, as in no. 6924. In some subjects the tendency is not apparent.

Occasionally, as in no. 6889, the increase of "voluntary" effort—due to the diminution of apprehension, distraction of surroundings, etc., and the increase of interest—is so great that the performance steadily improves in spite of the deleterious effect of depletion of oxygen, until the subject approaches cardiovascular collapse. In this connection, it may be remarked that one of the most frequently noted effects of diminution of the oxygen-supply, is a constriction of the field of attention, which facilitates the application of effort exclusively to the task in hand. If the task is sufficiently simple, improvement in performance may result.

A brief comment is presented with the graphic record in each case, and should assist materially in interpretation of the record.

The reader is earnestly cautioned against drawing conclusions from superficial inspection of the graphs. Contrary to the first visual impressions, the graphs do not give a quantitative indication of the extent of deterioration of performance. This fact will be made clear by consideration of the following facts:

1. It would be unfair to compare the performance of subjects having different degrees of dexterity, with a uniform standard.

2. Owing to the fact that the subjects are available for only a single sitting, it is not easy to establish a "normal" performance for each one, with which the performance under diminished oxygen may be compared. As was pointed out above, the initial performance and also the rate of improvement with practice, depend so largely on variable and uncontrollable factors that the subject's capability is not necessarily indicated by them.

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3. The rate of deterioration cannot be determined accurately by the slope of the latter part of the curve, as this depends in large part on the relative excellence of performance before deterioration began. Also, all experiments cannot be terminated in the same phase of the period of work. Indeed, not infrequently, a sudden collapse occurs during a period of rest. Also, the first appearance of symptoms of profound deterioration is separated from the symptoms of impending collapse by a temporal interval which varies widely among different subjects. Therefore, the time of removal is often determined by considerations of safety of the subject, and it frequently occurs too early in the work period to permit of completion of the last set of material. The performance during the last period being integrated over only a short time, the value of the last ordinate may be largely determined by fortuitous factors.

4. The deleterious effects of depletion of oxygen may be compensated for, for a time, by increased voluntary effort, and thus may fail of adequate representation in the graphic record.

With these precautions in mind the graphs, supplemented by clinical notes, are useful. The principal reason for presenting the results in this form is that graphic records, considered as wholes, are more easily comprehended than tables.

The writers believe that any work-test, made under the limiting conditions which apply to the present case, is open to the same objections, so that a reliable case-record is indispensable.

The heavy ordinate drawn through the breaks of the curves at the right of the graphs indicates the time of termination of the experiment. The straight line drawn from abscissal zero to this ordinate *approximates* the rate of oxygen depletion. Only the initial and final percentages being given, the actual rate cannot be accurately represented. The abscissal value to which the curve of psychological performance is continued by a broken line represents the estimated point at which complete breakdown would have occurred.

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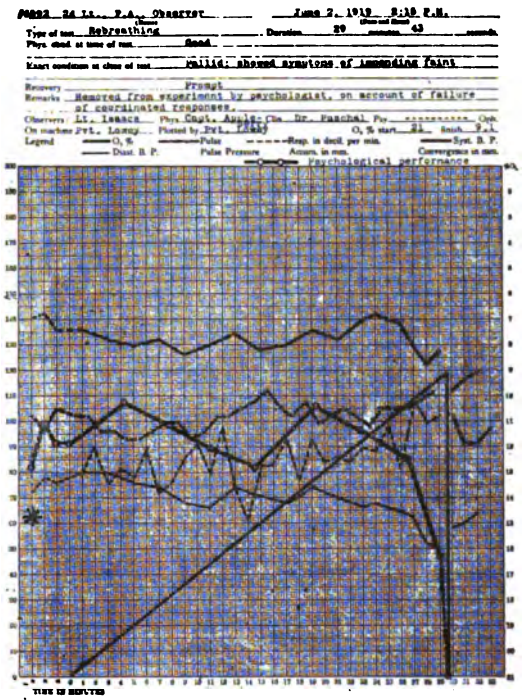
Type of test Rebreathing
Duration 29 43
Recovery Prompt
Remarks Removed from experiment by psychologist, on account of failure of coordinated responses.
Psychological performance

June 2, 1919. No. 6892, second lieutenant, observer, age unknown, single, ranchman.

Forty hours flying.

Run was preceded by 13 minutes' rebreathing during which the oxygen was replenished as consumed.

A distinct recovery from initial impairment followed by a steady decrease in performance, coincident with a diastolic drop. Removed for inefficiency after failure to find the next letter in several attempts. Muscular incoördination with marked tremor during 27th minute.

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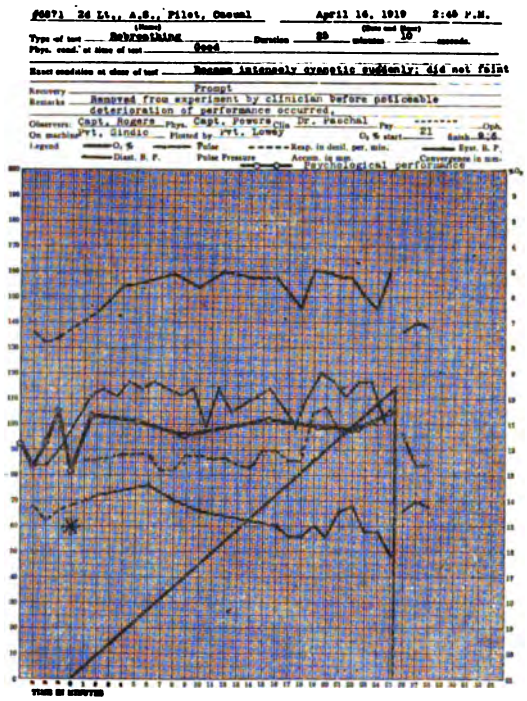

April 15, 1919. No. 6871, Second lieutenant, pilot, twenty-six, single, banker, two years college.

One hundred and fifty hours flying, 180 overseas, 3 over lines.

Suffering from loss of sleep.

Initial performance, rapid, accurate. Greatly disturbed when mouthpiece was inserted (set 5) but recovered. Removed by clinician on account of heart murmur before deterioration of behavior occurred.

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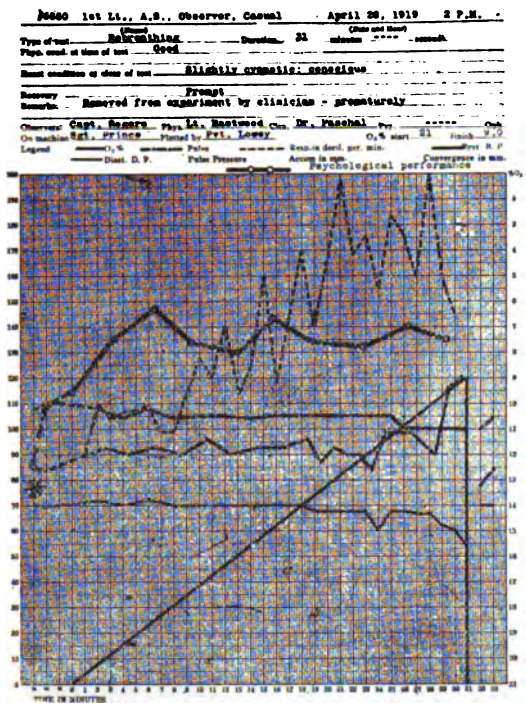

April 28, 1919. No. 6880, First lieutenant, observer, twenty-six, single. Nurseryman.

One hundred and ten hours flying, 8 combats, one crash, a poor landing; 18,500 feet for 10 minutes.

Physical condition good.

Run was preceded by 13 minutes' rebreathing during which the oxygen was replenished as consumed.

He attained high speed and accuracy which continued until the 18th minute, when he began to be less accurate, though speed did not suffer. He was prematurely removed at the 31st minute by the clinician.

This case shows the stimulation that occurs under low oxygen, the greatly increased speed being at the expense of accuracy.

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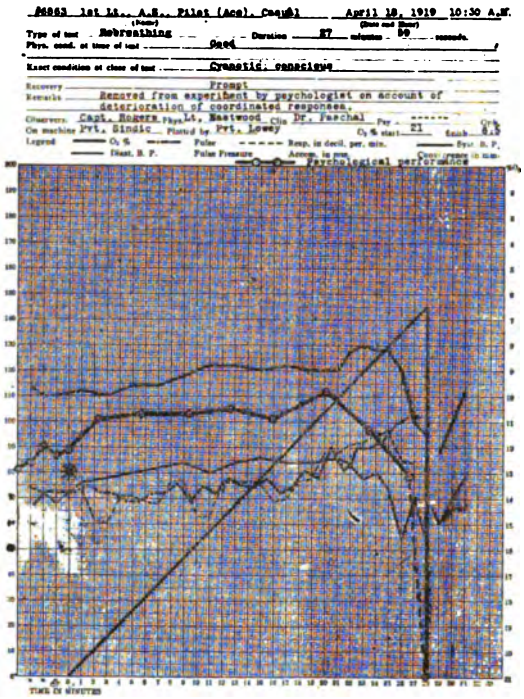
Type of test Rebreathing Duration 57 minutes 59 seconds.
Phys. cond. at time of test Good
Exact condition at close of test Cyanotic, conscious
Recovery Prompt
Remarks Removed from experiment by psychologist on account of deterioration of coordinated responses.
Observers Capt. Rogers Phys. Lt. Eastwood Chs Dr. Paschal
On machine Pvt. Sindic Plotted by Pvt. Lowy
Legend O2 % Pulse Resp. in decil. per. min.
Temp. R. P. Pulse Pressure Accomp. in mm.
Psychological performance
TIME IN MINUTES

April 18, 1919. No. 6863, first lieutenant, pilot, twenty-six, married, mining engineer, college education.

Three hundred hours flying. Ace.

Suffering from cold. Only 4 hours sleep previous night. Stale. Tired of flying. When taken off was troubled by insomnia.

Initial performance; accurate, fairly rapid. Steady improvement with practice. Deterioration expressed in diminished speed—beginning in 22d to 23d minute, and accompanied by controlled fall in diastolic blood pressure. No errors until last set. Letters quite legible to the last.

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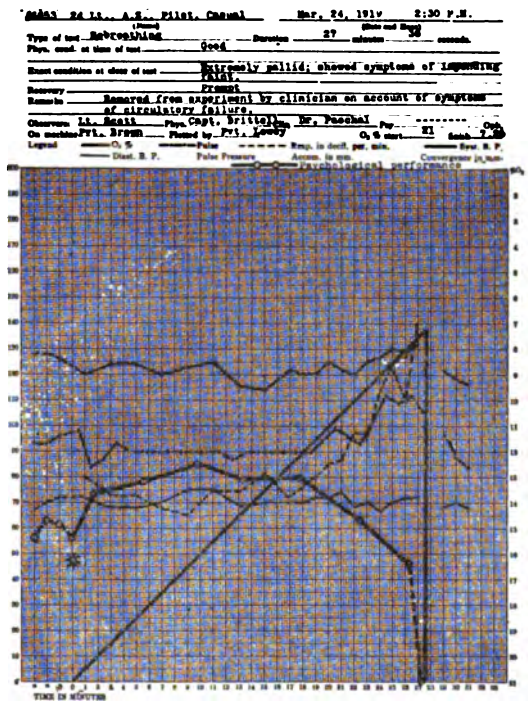

March 24, 1919. No. 6653, second lieutenant, Air Service, twenty-four, single, mechanic, 1 year high school.

One hundred hours flying, maximal altitude 18,000 feet for 10 minutes.

Physical condition good.

Initial performance only fair, due to slowness; few errors; slight disturbance when mouthpiece was inserted. Considerable improvement from practice during first half of test. Deterioration due to impaired muscular coördination, resulting in slowness; began at 21st to 22d minute between 10 and 11 per cent O_2. Greatly increased effort. Hard pressure with pencil from 21st minute—very tremulous at 26th. Unable to form letters after 27 minutes 13 seconds.

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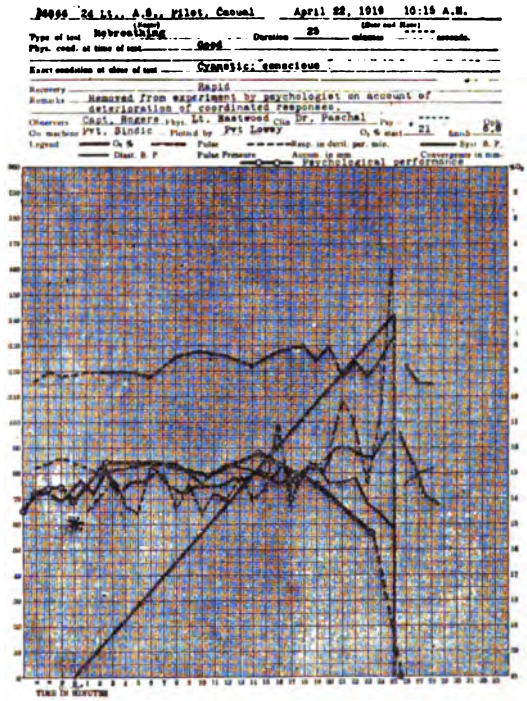

April 22, 1919. No. 6866, second lieutenant, pilot, twenty-two, single, merchant, 5 years Yale.

One hundred hours flying, all overseas. Maximal altitude 12,000 feet for 15 minutes.

Suffering from chronic cough.

Slow and accurate at start. Excited when mouthpiece was inserted (made 4 errors) in set 4. Improved in subsequent performance. Deterioration from 17th to 18th minute in both time and accuracy. Impairment of performance accompanied by diminution of diastolic blood pressure.

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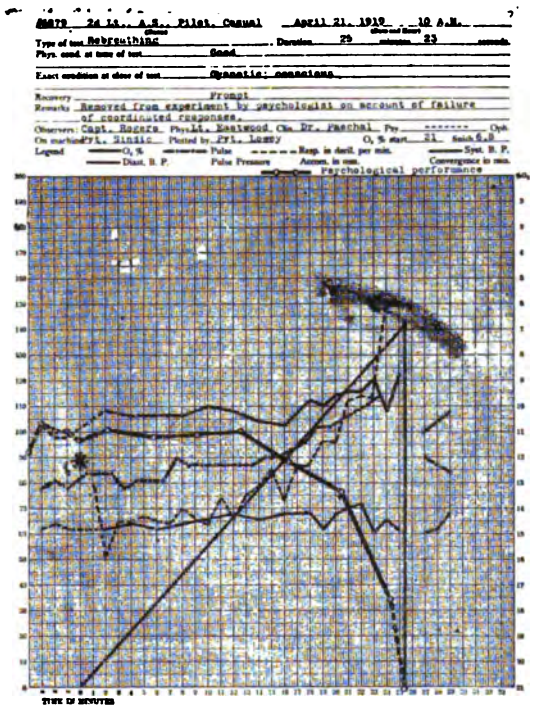
6879 2d Lt., A. S. Pilot. Casual April 21, 1919. 10 A.M.
Type of test Rebreathing. Duration 29 minutes 23 seconds.
Phys. cond. at time of test Good.
Exact condition at close of test Cyanotic; convulsions.
Recovery Prompt
Remarks Removed from experiment by psychologist on account of failure of coordinated responses.
Observers: Capt. Rogers Phys. Lt. Eastwood Ch. Dr. Paschal Psy. -------- Opt.
On machine Pvt. Sinsic Plotted by Pvt. Lowry O2 % start 21 finish 6.8
Legend O2 % Pulse Resp. in duct. per min. Syst. B. P.
Diast. B. P. Pulse Pressure Ataxia, in mm. Convergence in mm.
Psychological performance
TIME IN MINUTES

April 21, 1919. No. 6879, second lieutenant, pilot, twenty-four, single, bacteriologist.

Two hundred and fifty hours flying, 100 hours over lines; several combats; 24,000 feet for 5 minutes without oxygen, and 20,000 feet for 45 minutes.

Physical condition good.

Run was preceded by 5 minutes' rebreathing during which the oxygen was replenished as consumed.

He attained his maximal performance almost at the start and maintained it up to the 15th minute, when deterioration began and continued up to the end. He was taken off by the psychologist at 25:23 as inefficient when he had become very slow and was making numerous errors.

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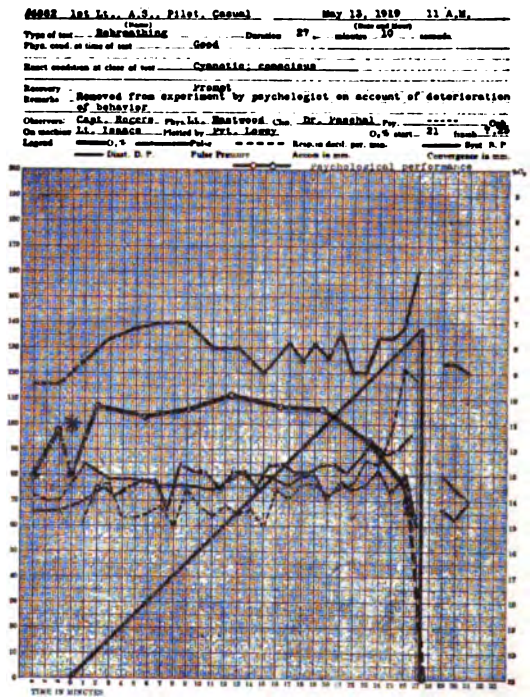

May 13, 1919. No. 6882, first lieutenant, pilot, twenty-seven, single, personnel service.

One hundred hours flying, none over lines. One crash, poor landing.

Physical condition good.

Run was preceded by 5 minutes' rebreathing during which the oxygen was replenished as consumed.

The performance was constant until the 22d minute when a retardation occurred accompanied by a rise in respiratory rate and pulse pressure. The speed of performance decreased and the number of errors increased until the last minute, when he almost ceased working altogether. He was taken off by the psychologist at 27:10 for inefficiency.

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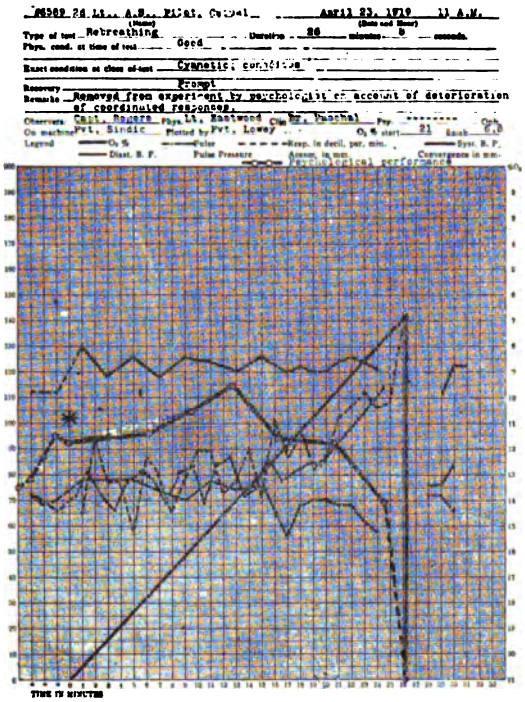

April 23, 1919. No. 6589, second lieutenant, Air Service, pilot, twenty-eight, single, superintendent of public schools.

No service overseas. One hundred and thirty hours flying, maximal altitude 10,000 feet for 15 or 20 minutes.

Suffering from nasal catarrh; condition otherwise good.

Well composed. No disturbance when mouthpiece was inserted. Considerable improvement with practice during first half of test. Deterioration began in region of 16th to 17th minutes, 12 to 11 per cent O_2. Completely inefficient at 26th minute (6.8 per cent O_2).

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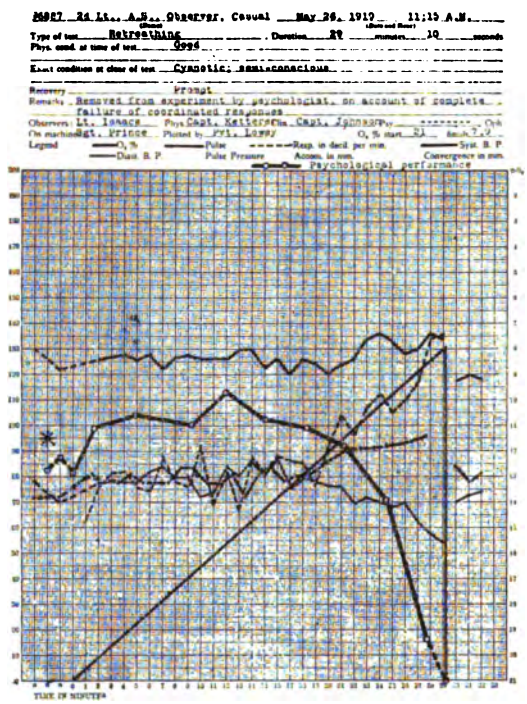

May 26, 1919. No. 6887, second lieutenant, observer, twenty-five, single, banker

Seventy-five hours flying. Maximal altitude 15,000 feet for 5 minutes.

Run was preceded by 13 minutes' rebreathing during which the oxygen was replenished as consumed.

Both the speed and accuracy were somewhat low. Final deterioration began at the 18th minute coincident with the beginning of a controlled drop in diastolic blood pressure, reaching complete inefficiency at 29:10, when he was taken off by the psychologist. He was then deficient in motor control and at the end had superimposed four letters.

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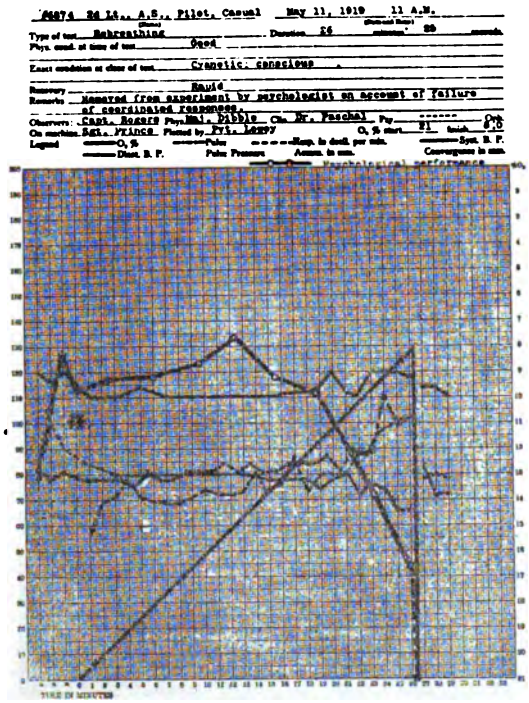
#6874 2d Lt., A.S., Pilot, Casual May 11, 1919 11 A.M.
Type of test Rebreathing Duration 26 minutes 00 seconds
Phys. cond. at time of test Good
Exact condition at close of test Cyanotic, conscious
Recovery Rapid
Remarks Removed from experiment by psychologist on account of failure of coordinated responses
Observers Capt. Rogers Phys. Maj. Dibble Ch. Br. Paschal Psy. ------ Cyl.
On machine Sgt. Prince Plotted by Pvt. Lacey O, % start. 81 finish 8.6
Legend O, % Pulse Resp. in deal. per min. Syst. B. P.
Dias. B. P. Pulse Pressure Accom. in min. Convergence in min.
Psychological performance
TIME IN MINUTES

May 11, 1919. No. 6874, second lieutenant, pilot, twenty-three, single, clerk. One hundred hours flying, 30 overseas, none over lines. One crash from forced landing.

Physical condition good but slept poorly the night before.

Run was preceded by 5 minutes' rebreathing during which the oxygen was replenished as consumed.

A rapid improvement over the initial performance with a decided drop when mouthpiece was introduced. There was a steady improvement up to the 13th minute, at which time a very high speed was attained. From this point, coincident with a rise in pulse rate and pulse pressure, there was a drop to the level of the previous sets and after the 18th minute a rapid decline of both speed and accuracy. At 26:25 he had become inefficient through deterioration of both attention and muscular coördination.

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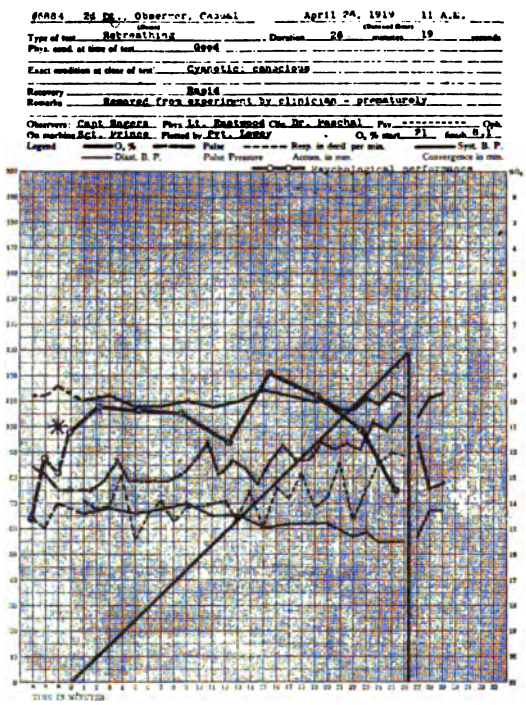
#6884 26 Yr., Observer, Casual April 26, 1919 11 A.M.
Type of test Rebreathing Duration 20 minutes 19 seconds
Phys. cond. at time of test Good
Exact condition at close of test Cyanotic; convulsive
Recovery Rapid
Remarks Removed from experiment by clinician - prematurely
Observers: Capt Rogers Phys Lt. Eastwood Clin Dr. Paschal Psy
On machine Sgt. Prince Plotted by Pvt. Lemay O2 % start 21 finish 8.1
Legend O2 % Pulse Resp. in deci per min. Syst. B. P.
Diast. B. P. Pulse Pressure Accom. in mm. Convergence in mm.
Psychological performance
TIME IN MINUTES

April 26, 1919. No. 6884, second lieutenant, Air Service, observer, twenty-five, single, student, 5 years State Normal; 2 years basketball.

One hundred and twenty-five hours flying, 40 overseas. No record of combats or crashes. Maximal altitude 15,000 feet for 120 minutes.

Initial performance; accurate, rapid. Disturbed by introduction of mouthpiece but increased speed with practice during the work in normal air. Significant disturbance at 11 to 12 per cent oxygen in the form of decreased speed. This was more than compensated for by the exercise of greatly increased effort, which later ceased to be effective. Experiment terminated prematurely by clinician.

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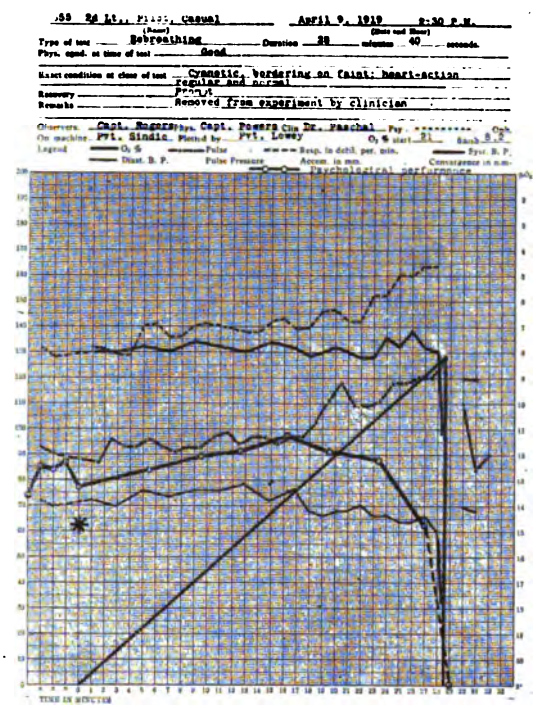
355 2d Lt., Pilot, Casual April 9, 1919 2:30 P.M.
Rebreathing Duration 28 minutes 40 seconds.
Good
Cyanotic, bordering on faint; heart-action regular and normal
Prompt
Removed from experiment by clinician
Capt. Rogers Capt. Powers Dr. Paschal
Pvt. Sindic Pvt. Lowey
Pulse
Pulse Pressure
Psychological performance
TIME IN MINUTES

April 9, 1919. No. 355, second lieutenant, Air Service, pilot, twenty-four, married, student.

Returned as casual from overseas. Seventy-six hours of flying, 30 hours overseas, 1½ hours over lines. Maximal altitude, 12,000 feet for 120 minutes.

Present health good. Disturbed by insertion of mouthpiece before 5th set presented before rebreathing began. (This is plotted by mistake on the zero abscissal coördinate.) Recovery and improvement. Gradual deterioration from 16th minute, 11 to 12 per cent O_2. In fainting condition when removed. Inefficiency concurred with circulatory break. Duration of test, 28 minutes 40 seconds.

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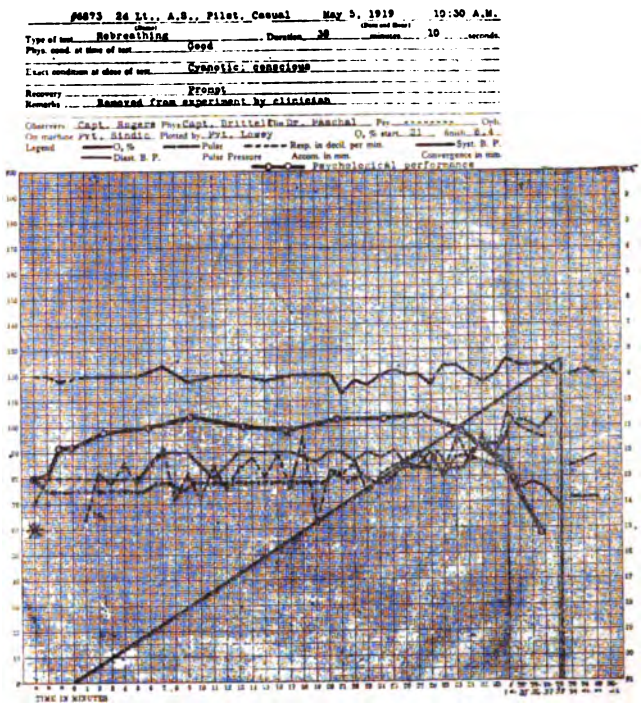
6873 2d Lt., A.S., Pilot, Casual May 5, 1919 10:30 A.M.
Type of test Rebreathing Duration 38 minutes 10 seconds.
Phys. cond. at time of test Good
Exact condition at close of test Cyanotic, conscious
Recovery Prompt
Remarks Removed from experiment by clinician
Observers Capt. Rogers Phys. Capt. Brittel Dr. Paschal Per Orb.
On machine Pvt. Sindic Plotted by Pvt. Lowy O, % start 21 finish 8.4
Legend O, % Pulse Resp. in decil. per min. Syst. B. P.
Diast. B. P. Pulse Pressure Accom. in mm. Convergence in mm.
Psychological performance
TIME IN MINUTES

May 5, 1919. No. 6873, second lieutenant, pilot, twenty-seven, single, manager of store.

One hundred and twenty-five hours flying, 60 overseas, none over lines, no combats or crashes.

Run was preceded by 13 minutes' rebreathing during which the oxygen was replenished as consumed.

Physical condition good. Rated as stale.

Initial performance fairly rapid, accurate. Reached general level of performance in 5th set. General retardation, with not over 6 per cent errors from 26th minute.

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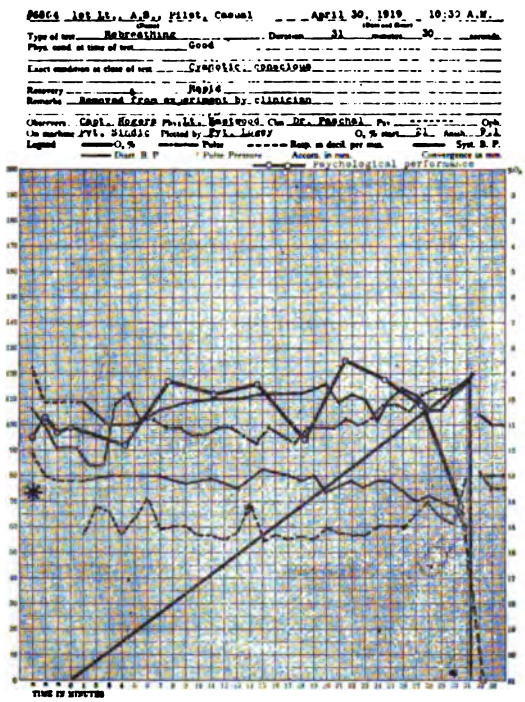

April 30, 1919. No. 6864, first lieutenant, Air Service, twenty-two, single, undergraduate student.

Five hundred hours flying, 450 overseas, 100 over lines; no crashes or combats reported.

Run was preceded by 13 minutes' rebreathing, during which the oxygen was replenished as consumed.

Initial performance, rapid, accurate. Improvement from 7th minute under diminution of O_2. Confused at beginning of set presented in 17th minute—made several errors which he paused to correct. Next performance improved. Inaccuracy manifested from 23d to 24th minute and increasing slowness from 26th to 27th. Removed by clinician before reaching complete psychological breakdown.

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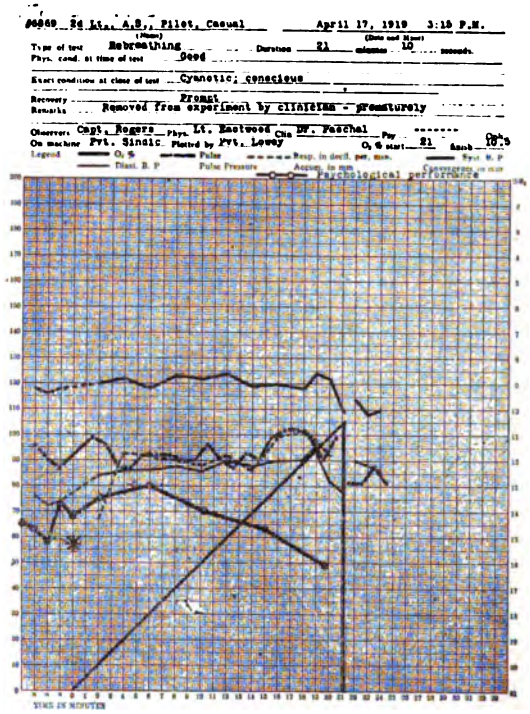

April 17, 1919. No. 6869, second lieutenant, pilot, twenty-three, single, automobile business, 2 years high school.

Two hundred hours flying, all overseas.

Physical condition good, but rated as stale.

Initial performance; slow, accurate, considerable improvement with practice. Deterioration from 8 to 9 minutes, due to decreased speed. When removed, subject was superposing one printed letter on another—his work in the last set being almost illegible. This effect preceded diminution of blood pressure

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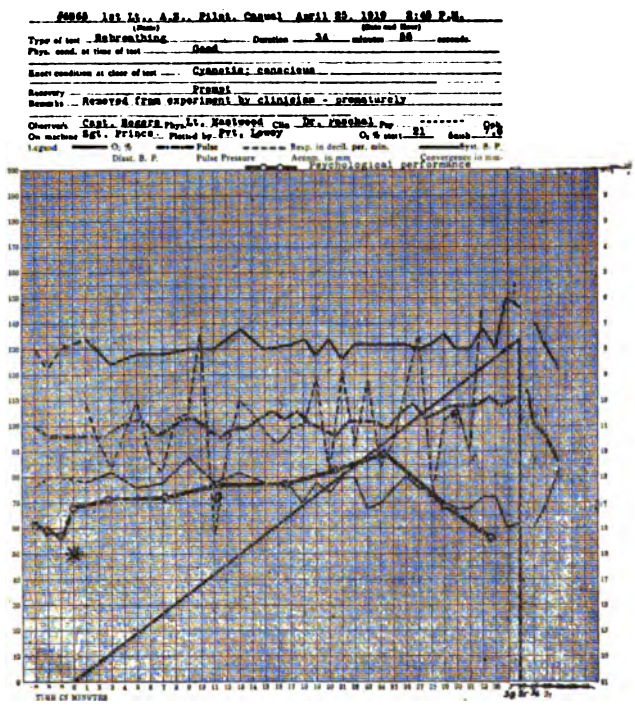

April 25, 1919. No. 6865, first lieutenant, Air Service, pilot, twenty-six, single, salesman, high school education, football team, 1912–1913.

Five hundred hours flying, 425 overseas; none over lines; no crashes.

Slightly disturbed by insertion of mouthpiece. Unusually slow from beginning; accurate, speed increased slowly with practice. Deterioration began in neighborhood of 11 per cent oxygen, in form of slowing of responses. Subject attempted to compensate by greatly increased effort. Experiment terminated prematurely by clinician.

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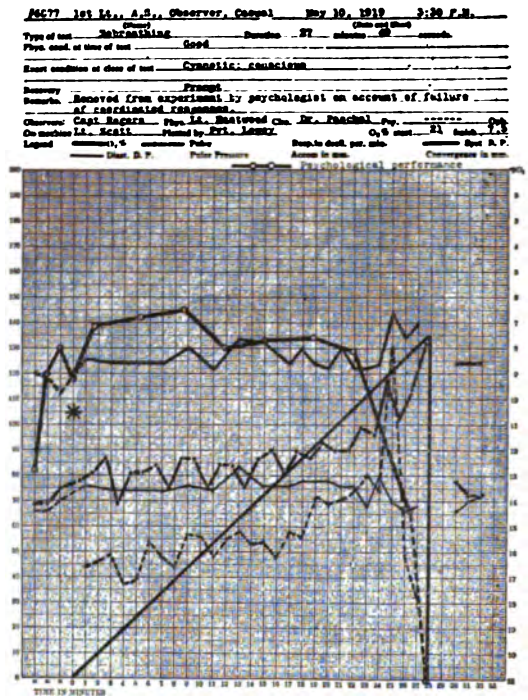

May 10, 1919. No. 6877, first lieutenant, observer, twenty-eight, single, civil engineer.

Seventy-five hours flying with 40 over lines, 8 crashes, 3 from poor landings and 5 from forced landings.

Physical condition good, but had little sleep the night before examination.

Run was preceded by 5 minutes' rebreathing during which the oxygen was replenished as consumed.

A high speed was soon attained somewhat at the expense of accuracy. There was a decrease in speed at the 11th minute to a rate which remained constant to the 22d minute, when the final slowing up began. He was taken off by the psychologist at 27:49 for inefficiency, three letters having been superimposed at the end and a number of mistakes had preceded. A controlled drop in the diastolic blood pressure occurred during the last series.

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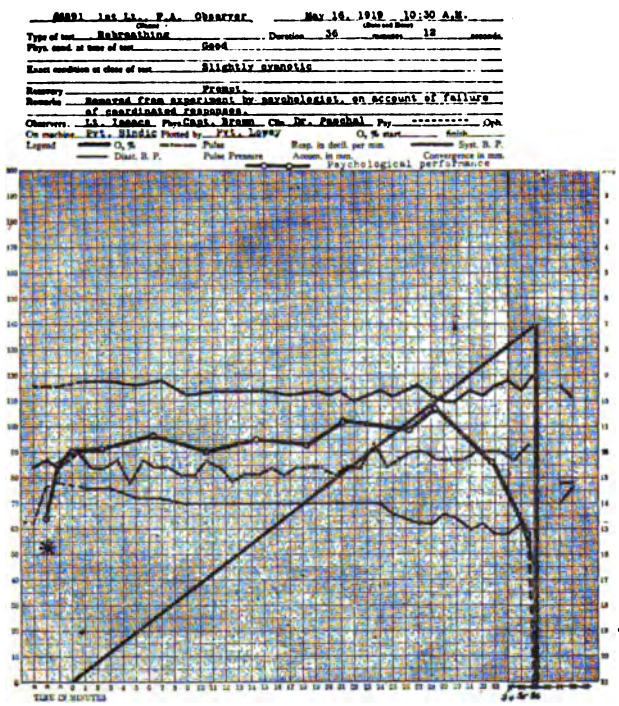
6891 1st Lt., P.A. Observer May 16, 1919 10:30 A.M.
Type of test Rebreathing Duration 36 minutes 12 seconds
Phys. cond. at time of test Good
Exact condition at close of test Slightly cyanotic
Recovery Prompt.
Remarks Removed from experiment by psychologist, on account of failure of coordinated responses.
Observers 1st. Isaacs Phy. Capt. Bramm Ch. Dr. Paschal Psy. Opr.
On machine Pvt. Biddle Plotted by Pvt. Lapsy
Legend O2 % Pulse Resp. in decil. per min. Syst. B. P.
Diast. B. P. Pulse Pressure Accom. in mm. Convergence in mm.
Psychological performance
TIME IN MINUTES

May 16, 1919. No. 6891, first lieutenant, pilot, twenty-seven, single, lawyer. Physical condition good.

Forty hours flying, none over lines, no crashes or combats.

Run was preceded by 13 minutes' rebreathing during which the oxygen was replenished as consumed.

There was a tendency toward increase in performance level until the last two series were reached, being partly due to decreased attention to the legibility of his writing and partly to increased attempt to speed. When removed he had become unable to make correct substitutions, this being coincident with a sudden drop in diastolic blood pressure.

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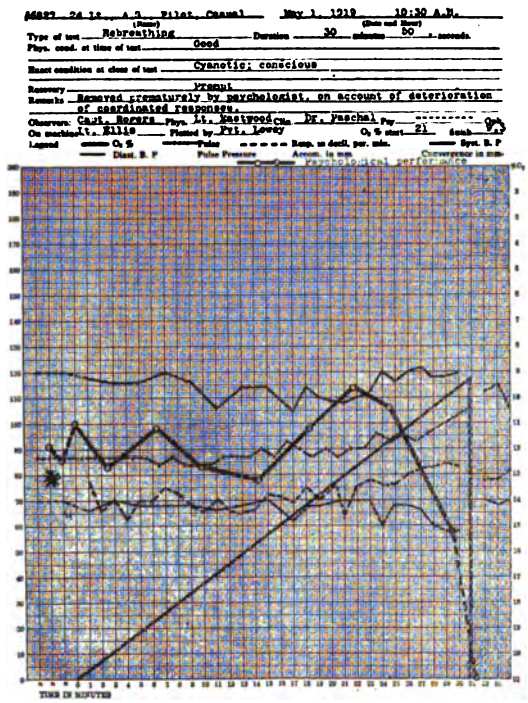

May 1, 1919. No. 6827, second lieutenant, Air Service, twenty-five, single, draftsman.

One hundred and seventy-five hours flying, 140 hours overseas, none over lines. No combats, or crashes.

Present condition good. Rated as "stale."

Initial performance: rather rapid, few errors. Effects of early practice inconsistent. Accurate and fairly rapid. Slight deterioration at about 15th minute, compensated for by increased effort. Deterioration began in 24th minute between 10 and 11 per cent O_2—due first to inaccuracies and later to loss of speed. Removed prematurely by psychologist. Record interesting as example of result of compensatory effort.

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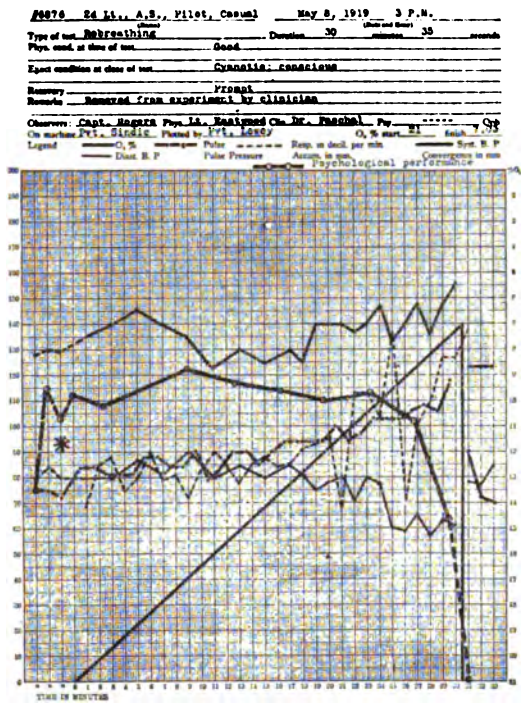
#6876 2d Lt., A.S., Pilot, Casual May 8, 1919 3 P.M.
Type of test Rebreathing Duration 30 minutes 35 seconds
Phys. cond. at time of test Good
Exact condition at close of test Cyanosis: conscious
Recovery Prompt
Remarks Removed from experiment by clinician
Observers: Capt. Rogers Phys. Lt. Eastwood Clin. Dr. Paschal Psy.
Clin. markings Pvt. Hindle Pianist Pvt. Lacey
Legend O, %
Dias. B. P
Pulse
Pulse Pressure
Resp. in decil. per min.
Psychological performance
Syst. B. P
TIME IN MINUTES

May 8, 1919. No. 6876, second lieutenant, pilot, thirty, single, electrical engineer.

Three hundred and fifty hours flying, all overseas, no crashes or combats, 24,000 feet for 20 minutes.

Physical condition good. Run was preceded by 5 minutes' rebreathing during which the oxygen was replenished as consumed.

This work was quite rapid and accurate, with an initial impairment coincident with a fall in diastolic blood pressure at 19 minutes and the final deterioration began at 23 minutes accompanied by another fall in the diastolic. A sudden break in the latter caused the clinician to take him off just before complete inefficiency was reached but not until he had shown a distinct deterioration in speed and legibility and had made many errors and repetitions.

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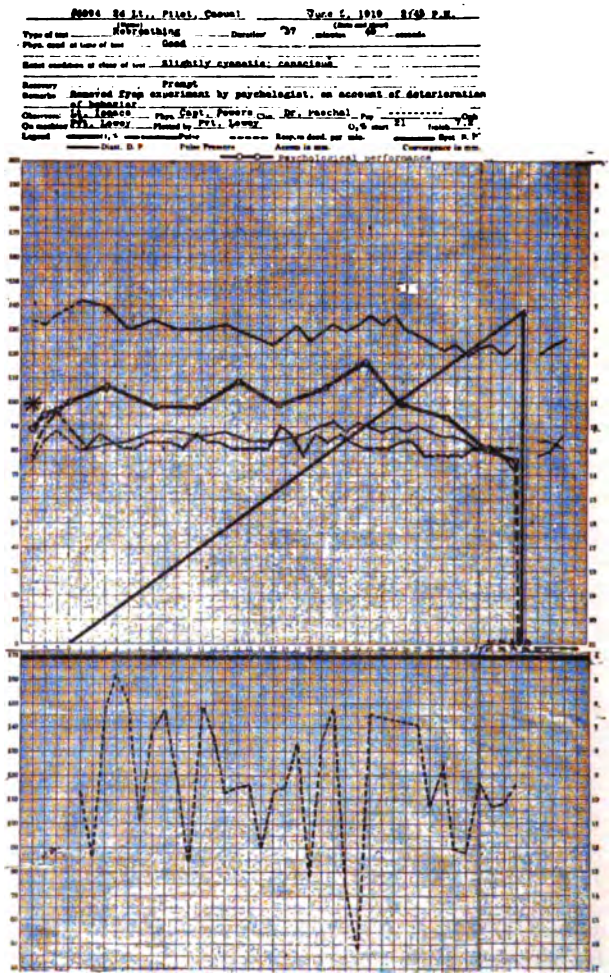

June 5, 1919. No. 6894, second lieutenant, pilot, twenty-nine, single, ranch manager.

One hundred hours flying, one crash.

Physical condition good but had a slight headache.

Run was preceded by 13 minutes' rebreathing during which the oxygen was replenished as consumed.

Only moderately fast but quite accurate. In the 14th minute he showed distinct initial impairiment of muscular coördination and attention. After the 11th series, he showed progressive decrease in both speed and accuracy until the end. He was taken off for inefficiency at 37:45. He was still making some correct substitutions but was finding the operations, including the writing, very slow and difficult. A controlled drop of diastolic blood pressure was occurring during the last few series.

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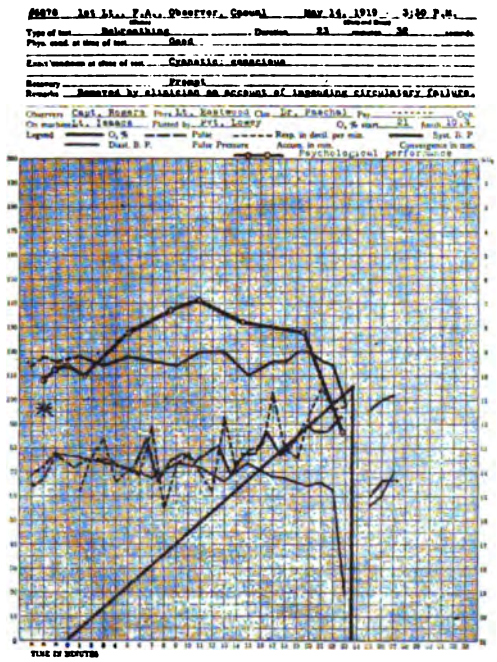

May 14, 1919. No. 6878, first lieutenant, observer, thirty-four, married, stock broker.

One hundred hours flying, 30 hours over lines, 3 combats, 2 credits, was shot down; 18,000 feet for 2 hours.

Physical condition good. Run was preceded by 13 minutes' rebreathing during which the oxygen was replenished as consumed.

Mouthpiece in place at start. A steady increase in speed was shown during the first 13 minutes of the run, when a steady decline began. He was taken off at 23:32 by the clinician when both the systolic and diastolic blood pressures fell. While accuracy was still fair, a fall in the rate of substitution had accompanied the fall in blood pressure.

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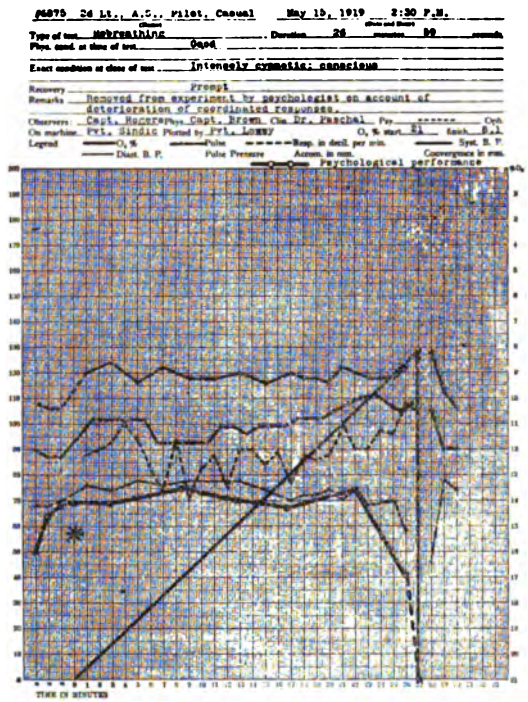
6875 2d Lt., A.S., Pilot, Casual May 15, 1919 2:30 P.M.
Type of test Rebreathing Duration 28 minutes 30 seconds
Phys. cond. at time of test Good
Exact condition at close of test Intensely cyanotic; conscious
Recovery Prompt
Remarks Removed from experiment by psychologist on account of deterioration of coordinated responses.
Observers Capt. Rogers Phys. Capt. Brown Chs Dr. Paschal Psy. Cyan
On machine Frt. Sindic Plotted by Frt. Lowry O, % start 81 finish 8.1
Legend O, % Pulse Resp. in decil. per min. Syst. B. P.
Diast. B. P. Pulse Pressure Accom. in mm. Convergence in mm.
Psychological performance
TIME IN MINUTES

May 15, 1919. No. 6875, second lieutenant, pilot, twenty-nine, married, merchant.

Three hundred hours flying, 175 overseas, none over lines. One crash due to forced landing.

Physical condition good. Machine set for 28-minute run with 5-minute normal period.

This was a slow, steady performance, showing a slight diminution of accuracy in the 16th and 17th minutes and a pronounced retardation with still greater inaccuracy in the last series. This was accompanied by a fall in the diastolic blood pressure. At the end, he ceased working quite suddenly with but slight preliminary deterioration of either the motor or attention processes. As the subject apparently made little effort and at no time made a good performance, the progress of deterioration is not clearly marked.

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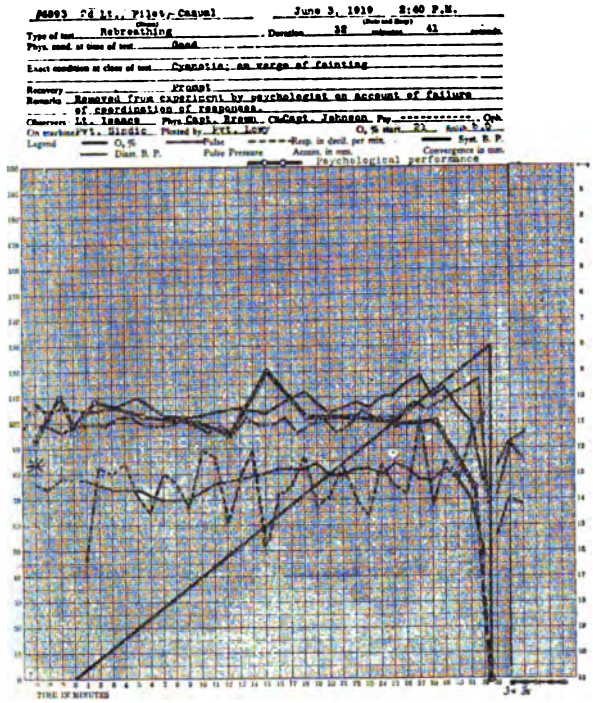
6893 2d Lt., Pilot, Casual
June 3, 1919 8:00 P.M.
Type of test Rebreathing. Duration 38 minutes 41 seconds
Phys. cond. at time of test Good
Exact condition at close of test Cyanosis; on verge of fainting
Recovery Prompt
Remarks Removed from experiment by psychologist on account of failure of coordination of responses
Observers Lt. Isaacs Phys. Capt. Brown Capt. Johnson Psy. Orb.
On machine Pvt. Hindle Plotted by Pvt. Low O2 % start 23 finish 9.2
Legend O2 % Pulse Resp. in decil. per min. Syst. B. P.
Diast. B. P. Pulse Pressure Accom. in mm. Convergence in mm.
Psychological performance
TIME IN MINUTES

June 3, 1919. No. 6893, second lieutenant, pilot, twenty-seven, single, mechanical engineer.

One hundred hours' flying. None over lines.

Physical condition good, but had lost sleep the night before examination.

Run was preceded by 13 minutes' rebreathing during which the oxygen was replenished as consumed.

A moderately fast and accurate performance up to the last series, when he became inefficient at the time of a break in systolic and diastolic blood pressure and pulse rate. He had been holding to his performance level by increased effort under low oxygen.

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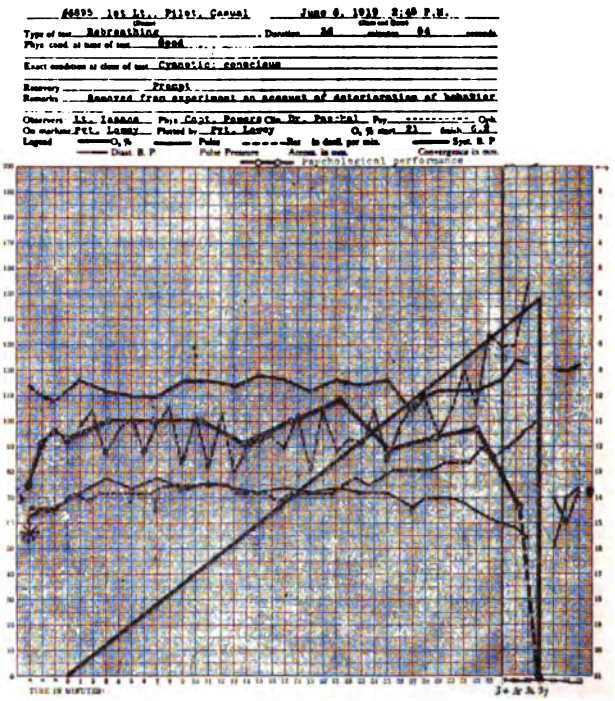

June 6, 1919. No. 6895, first lieutenant, pilot, twenty-six, single, chemical engineer.

Three hundred hours flying overseas, 100 over lines, 1 crash, night landing. 20,000 feet for 1½ hours; 15 combats, 3 credits.

Physical condition good. Run was preceded by 13 minutes' rebreathing during which the oxygen was replenished as consumed.

Slight initial impairment at 13th minute. Deterioration began at 11 per cent oxygen, but compensation took the form of increased effort. This effort became greater as time passed, but muscular incoördination and large number of errors appeared at the end of the last series. He was taken off the machine at the end of the series as he was becoming inefficient so rapidly that he could probably not have started the next series.

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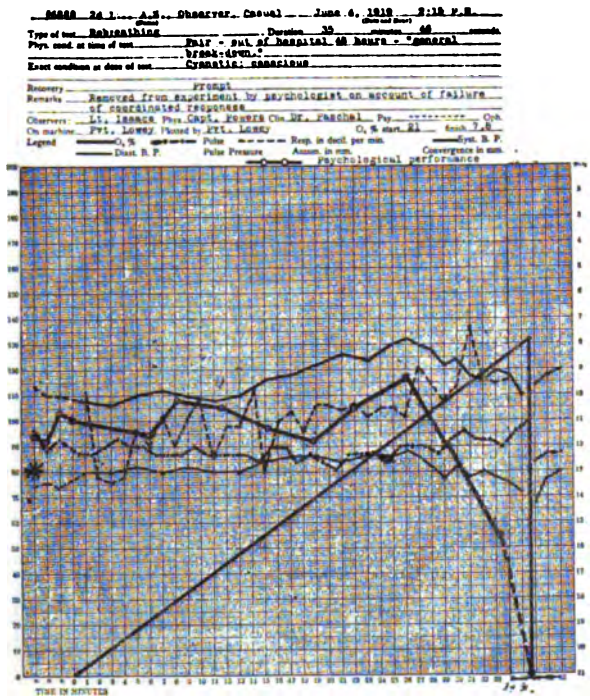
Type of test Rebreathing Duration 35 minutes 46 seconds
Phys. cond. at time of test Fair - out of hospital 48 hours - general break-down.
Exact condition at close of test Cyanotic; conscious
Recovery Prompt
Remarks Removed from experiment by psychologist on account of failure of coordinated response
Observers Lt. Isaacs Pilot Capt. Powers Clk Dr. Paschal Psy Opk
On machine Pvt. Lowry Piloted by Pvt. Lowry O, % start 21 finish 7.6
Legend O, % Pulse Resp. in decl. per min. Syst. B. P.
Diast. B. P. Pulse Pressure Anoxm. in mm. Convergence in mm.
Psychological performance
TIME IN MINUTES

June 4, 1919. No. 6888, second lieutenant, observer, twenty-five, married, structural engineer.

Two hundred and fifty hours flying with very little over lines.

Physical condition: Just out of hospital following general nervous breakdown, but was feeling fit at time of examination. Run was preceded by 13 minutes' rebreathing during which the oxygen was replenished as consumed.

The compensation in this case was through increased effort, following the appearance of an initial impairment in the eighteenth minute. Under further diminution of oxygen he greatly increased his performance, but it declined steadily after the 27th minute. He was taken off by the psychologist at the moment of a psychological break.

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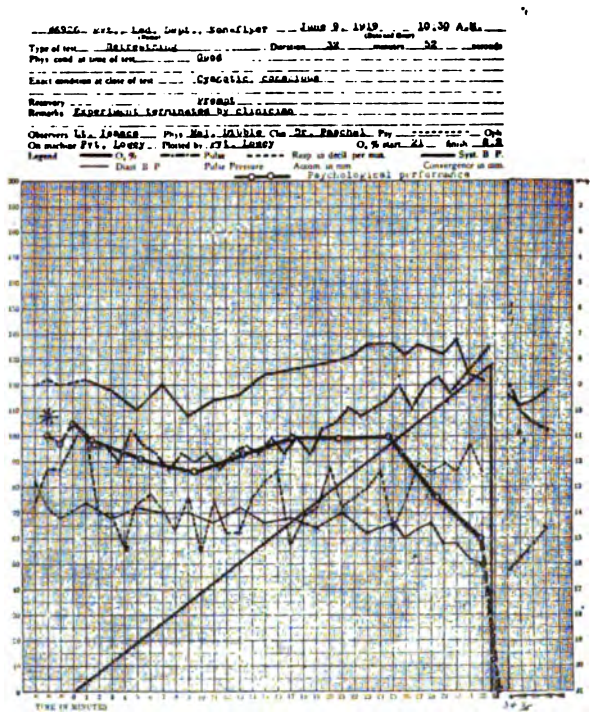

June 9 1919. No. 6926, private, Medical Department, twenty, photographer, non-flyer.

Three-quarter hours in air as passenger. Run was preceded by 13 minutes' rebreathing during which the oxygen was replenished as consumed.

Had previously transliterated all the material *once* under normal conditions: hence there is little improvement with practice.

Performance showed a slight and gradual decline in the early stages, with subsequent steady improvement due to increased effort. Deterioration began in the 24th and 25th minutes between 11 and 12 per cent oxygen, and increased rapidly until the experiment was terminated by the clinician.

The changes in the systolic blood pressure present an interesting parallel to the fluctuations in performance. The reading in the 7th minute was made during a period of rest.

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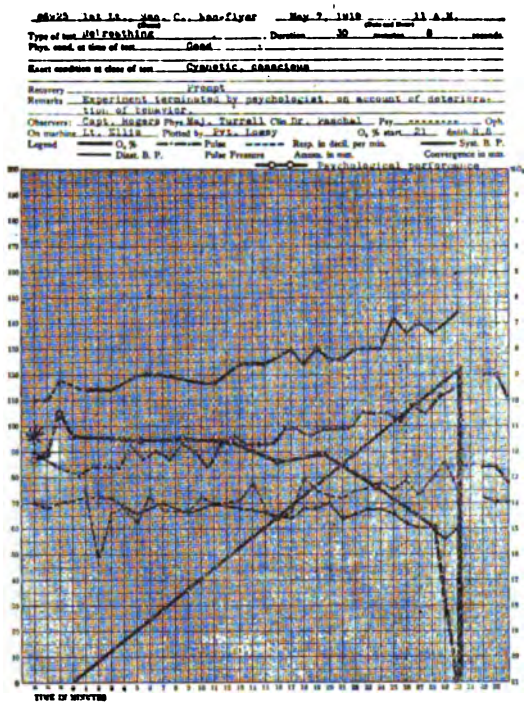
Type of test Rebreathing
Duration 30
Phys. cond. at time of test Good
Exact condition at close of test Cyanotic, conscious
Recovery Prompt
Remarks Experiment terminated by psychologist, on account of deteriora-
tion of transcription
Observers: Capt. Haggard Phys. Maj. Turrell Obs. Dr. Paschal Psy.
Ophs.
On machine Lt. Ellis Plotted by Pvt. Lowry O, % start 21 finish 8.6
Legend O, % Pulse Resp. in decil. per min. Syst. B. P.
Diast. B. P. Pulse Pressure Ammon. in mm. Convergence in mm.
Psychological performance
TIME OF MINUTES

May 7, 1919. No. 6925, first lieutenant, Sanitary Corps, single, thirty, psychologist, non-flyer.

One hour in air as passenger. Run was preceded by 13 minutes' rebreathing during which the oxygen was replenished as consumed.

Had previously transliterated all the material once under normal conditions. Hence little improvement over initial performance was shown.

A slight and transient impairment occurred during the 15th and 16th minutes and partially overcome by increased effort. Compensation was effective for only a short time. Subject was near collapse at time of removal.

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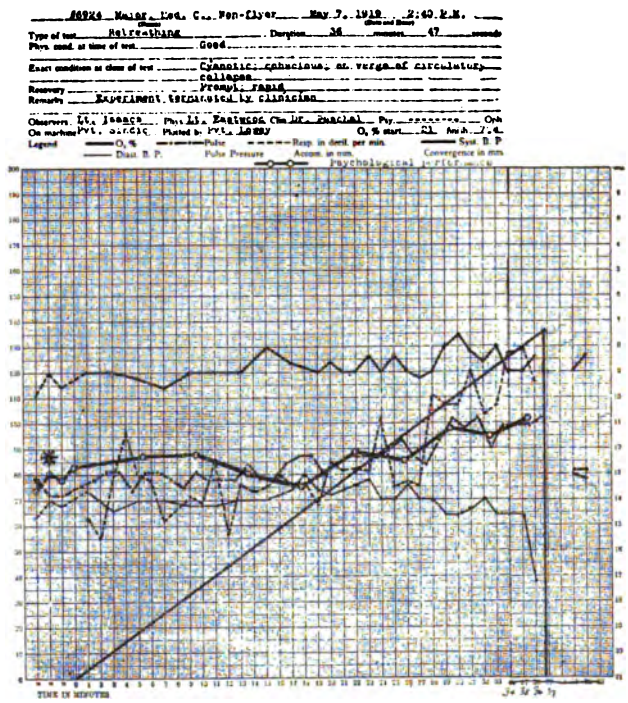
6924 Major Med. C. Non-flyer May 7, 1919 2:40 P.M.
Type of test Rebreathing Duration 36 47 seconds
Phys. cond. at time of test Good
Exact condition at close of test Cyanotic; conscious, at verge of circulatory collapse
Recovery Prompt; rapid
Remarks Experiment terminated by clinician
Observers Lt. Isaacs Pvt. 1c. Eastman Chs for Paschal Psy Obs
On machine Pvt. Sorgis Plotted by Pvt. Lasky O2 % start 21 finish 7.4
Legend O2 % Pulse Resp. in deci. per min. Syst. B. P.
Diast. B. P. Pulse Pressure Accom. in mm. Convergence in mm
Psychological
TIME IN MINUTES

July 7, 1919. No. 6924, major, Medical Corps, twenty-nine, married, flight surgeon, non-flyer.

Physical condition good. Run was preceded by 13 minutes' rebreathing during which the oxygen was replenished as consumed.

Very slow but very accurate, reaching level quite early. An initial effect, in the form of retardation appeared in the 17th and 18th minutes, but was overcome until 11 per cent oxygen was reached. At this point a controlled drop in diastolic blood pressure began. Compensation then took the form of increased effort and owing to the type of the early performance, this was more than sufficient to overcome the deteriorating effects of low oxygen, with the result that his performance was higher at the end of the run than at any previous time. He was taken off by the clinician when making his best performance because of a sudden drop in the diastolic blood pressure, which indicated that circulatory failure was impending.

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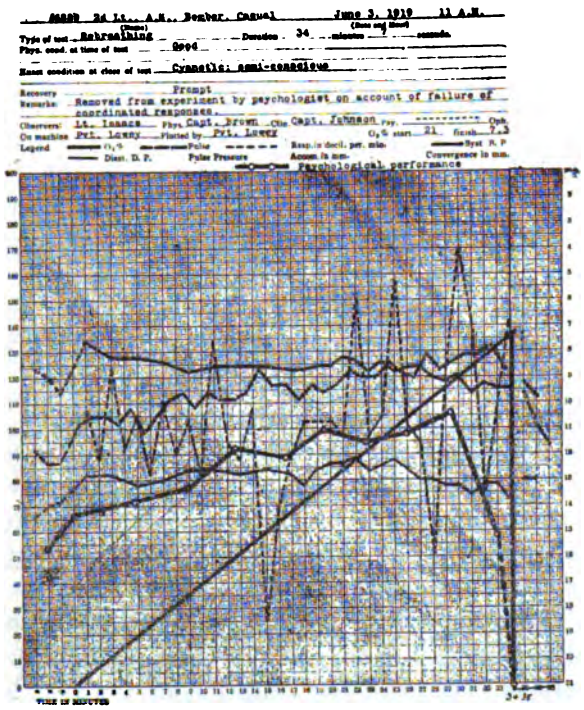

June 3, 1919. No. 6889, second lieutenant, bomber, twenty-six, single, construction engineer.

Sixty hours flying, none over lines. Physical condition good.

Run was preceded by 13 minutes' rebreathing during which the oxygen was replenished as consumed.

Owing to the low speed and the only fair accuracy, a steady increase in performance was shown until the later stages of the run. Increase of effort compensated for impairment until the final series. When the experiment was terminated he was repeating two letters over and over again, and was on the verge of fainting. (The cardiovascular failure occurred after the last systolic blood pressure reading was recorded.)

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INTERPRETATION OF RESULTS

The assertion has been made[4] that in some early experiments with this type of test, the performance seemed to be modified to an important extent by disturbances of accommodation and of oculomotor adjustment.

We were not successful, in the work reported in this paper, in obtaining evidence in support of this view. A supplementary experiment was made by Capt. Johnson, for the purpose of measuring disturbances of vision due to these factors. The results are not yet ready for publication, but are entirely negative. The characteristics of the records obtained indicate that the disturbances were more probably attentional than sensory. A similar uncertainty exists regarding all the rest of the work done directly on sensory process as affected by low oxygen.

One of the present authors has repeatedly reacted in experiments on the effects of oxygen-deprivation, and has noted that as collapse approaches, there is a great depression of vision; but it is manifested, not in the blurring of outlines of objects, but in a general darkening of the visual field and an intermittent cessation of all visual experiences (including that of darkness) together. This is especially marked in tests in which pure nitrogen is breathed instead of air, as the effect occurs very quickly and is clearly noticed; and the duration of the test being very short, the effect is more readily appraised afterwards than in the case of longer experiments. The other author has reacted in fewer experiments but his experience is very similar. A number of other subjects report a similar observation; and records made in a continuous, self-recording, test of visual acuity show the same tendency.[5]

[4] Manual of Medical Research Laboratory; Washington, Government Printing Office, 1918; p. 181. Also: Air Service Medical; Washington, Government Printing Office, 1918; p. 311.

[5] The authors do not regard this evidence as conflicting with the early diplopia reported by Dunlap as having been observed on himself and other subjects. (Cf. reference cited in footnote 1, above.) Dunlap's observation has received ample corroboration under the conditions under which it was made.

In the work reported in this paper, all the subjects were free from phorias or oculomotor imbalance, or possessed these defects in negligibly small degree. They

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In the substitution-test, subjects were frequently observed to stare at the test-card for several seconds before they initiated the recording movements, but the delay suggested muscular inhibition more strongly than anything else. The increased time required for performance in the later stages of the experiment is due largely to loss of muscular control. In several cases the subject was removed from the experiment for the reason that he was unable to form a letter; in other cases, because he was unable to put the pencil to the paper.

In the opinion of the writers, the test has considerable usefulness as a means of qualitative demonstration of the effects of low oxygen and of drugs. In experienced hands, it might be an acceptable substitute for the routine tests now employed in the classification of aviators although the detection of initial and intermediate disturbances is not quite as easy as in the official test. Its simplicity recommends it highly for exploratory work under external or physiological conditions which produce an effect of considerable magnitude in a relatively short time.

probably had a slighter tendency toward development of diplopia under asphyxiation than less highly selected subjects might have.

Besides the question of individual differences is the fact that in the official test the subject's responses are made to stimuli outside his control, irregular as to order and time of appearance, limited as to duration, variable as to position in all three planes, and having a rather wide separation. Adequacy of response therefore depended on a more highly coordinated type of eye-movement than was demanded by the conditions of this experiment.

In the experiment made directly on vision, the subjects, even in advanced stages of oxygen-deprivation, had little difficulty in maintaining steady fixation and accommodation through periods of three minutes. These observations being made monocularly, diplopia did not enter.

One would suspect that the maintenance of fixation on a single stationary object, or the alternation of fixation between two stationary objects in a relatively unlimited time, is less likely to induce and to exhibit diplopia than the more trying conditions of the official test.

While we do not contend that tendencies toward impairment of accommodation, convergence and muscular balance were absent in the present test, we obtained no evidence, either from the subjects' description or behavior, that these factors were operative in producing the results obtained.

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CONCLUSION

The results of the tests indicate that the simpler sensorimotor reflexes, as a rule, do not exhibit noticeable impairment until deterioration of the more highly organized responses has reached an advanced stage. Perhaps an adequate hypothetical explanation is that the deleterious influence of low oxygen on the subject's responses is largely, if not principally, due to interference at the synapses. On such an hypothesis one would expect that the processes which first show impairment are those which involve the highest degree of organization of neuromuscular arcs.

APPENDIX

The twenty sets of codes and test-material used in the foregoing work are reproduced below. A word regarding their genesis may be useful, as a very large amount of material was assembled, tried and rejected for important irregularities in the distribution of difficulty, before the method of compiling this material was developed. The first original attempt at compilation of work-material which is free from this objection will probably surprise the student by its difficulty.

The following rules were adopted for the construction of the codes. They were strictly followed, except for a few errors due to inadvertence, the correction of which would have necessitated recompilation of a large part of the material.

1. No code is systematically derived from another.
2. Every letter in the alphabet shall be represented by a different letter in the code.
3. No letter in the code is to be substituted for its immediately preceding letter in the alphabet.
4. Habitual sequences (e.g., OK, QU, etc.), are to be avoided, as are also sequences tending to form considerable parts of words.
5. A given letter is not to be substituted for the same letter more than twice in the 20 codes.

Subject to these limitations, the sequence of letters in each code was determined by a pack of shuffled cards.

The first set of work material was prepared by shuffling a pack of 52 cards containing two complete alphabets, and using the sequence of letters determined in this way.

The original set of material was then transliterated into code 1; and the result into code 2, etc. The last two letters of each set were discarded.

In the earliest of the experiments described in this report, some of the subjects discovered that the material presented with one code, became after transliteration the material presented with the next code. This discovery proved distracting as the subject was tempted to compare his performance by the card, to the neglect of the task. For this reason the various sets were thereafter given in shuffled order, and this source of disturbance disappeared.

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The several sets of material are presented below as shown on the cards actually used. The alphabet is given in the first line, the code in the second, and the material to be transliterated, below.

(1)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
P X G Z D K M B T F O E A Q L U Y J V H N I R C S W

D J E A S R N U M V P X H Y I T G L Z W O K C Q F B G P I X
W V U Q N C E O Y M K Z D T F S B G A R

(2)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
Q V T W Y N F O U X P Z J L A C I E R D S G B H K M

Z F D P V J Q N A I U C B S T H M E W R L O G Y K X M U T C
R I N Y Q G D L S A O W Z H K V X M P J

(3)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
Z O A R H S J M F D V G T X N B W L K E Q C Y P I U

M N W C G X I L Q U S T V R D O J Y B E Z A F K P H J S D T
E U L K I F W Z R Q A B M O P G H J C X

(4)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
X Z B G S D Y U M T V O K W I F N L H J P R E C A Q

T X Y A J P F G W Q K E C L R N D I O H U Z S V B M D K R E
H Q G V F S Y U L W Z O T N B J M D A P

(5)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
J H Z Q Y A O T B S U X W I G E V M C R L N D K P F

J C A X T F D Y E N V S B O L W G M I U P Q H R Z K G V L S
U N Y R D H A P O E Q I J W Z T K G X F

(6)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
S L X Z Q V C G B M W K U T E H Y A J P F O R I N D

S Z J K R A Q P Y I N C H G X D O W B L E V T M F U O N X C
L I P M Q T J E G Y V B S D F R U O K A

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(7)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
W S Q L V H R N Y I C J U A F M K P X G Z D B T E O

J D M W A S Y H N B T X G C I Z E R L K Q O P U V F E T I X
K B H U Y P M Q C N O L J Z V A F E W S

(8)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
U S K X R O W B G C M T V E L J Z N Q I Y D H A P F

I L U B W X E N A S G T R Q Y O V P J C K F M Z D H V G Y T
C S N Z E M U K Q A F J I O D W H V B X

(9)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
Q L J V I N W C S R H U Y E T G Z K D X P B M F O A

G T Y S H A R E U Q W I N Z P L D J C K M O V F X B D W P I
K Q E F R V Y M Z U O C G L X H B D S A

(10)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
G E V O Z S D C P U J I H F Y L K T M X N A Q R W B

W X O D C Q K I P Z M S E A G U V R J H Y T B N F L V M G S
H Z I N K B O Y A P T J W U F C L V D Q

(11)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
E D K B M T Q Z G X A P O F W L S C U Y J R N I H V

Q R Y O V K J P L B H M Z G D N A T U C W X E F S I A H D M
C B P F J E Y W G L X U Q N S V I A O K

(12)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
J G M V K E L N A B U H T S X Z F O W R Q C Y P D I

S C H W R A X L P D Z O V Q B F E Y J K N I M T U G E Z B O
K D L T X M H N Q P I J S F U R G E W A

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(13)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
O T M G Z X K A P E D F B V J H I N R C Y Q S U W L

W M N Y O J P H Z V I X C F G E K D B U S A T R Q L K I G X
U V H R P T N S F Z A B W E Q O L K Y J

(14)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
N A Y M X R V J O W H U Q I F T E G Z K D S P B L C

S B V W J E H A L Q P U M X K Z D G T Y R O C N I F D P K U
Y Q A N H C V R X L O T S Z I J F D W E

(15)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
X Z V K R M T O L U Y C I W S N B F J Q E A H D G P

Z A S P W X J N U E T D Q B H C M V K L G F Y I O R M T H D
L E N I J Y S G B U F K Z C O W R M P X

(16)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
H Y G Q L Z X D C V O J F K W A S M E B I U T N R P

P X J N H D U W E R Q K B Z O V I A Y C T M G L S F I Q O K
C R W L U G J T Z E M Y P V S H F I N D

(17)

A B C D E F G H I J K L M N O P Q R S T U V W X V Z
L E Y T B C Z R D W S O G P X K J I H V M F U N A Q

A N V K D Q I T L M S O Y P W U C H R G B F X J E Z C S W O
G M T J I X V B P L F R A U E D Z C K Q

(18)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
V N A L H U O S R Z G Y W Q J D C P X M K E I F B T

L P F S T J D V O G H X A K U M Y R I Z E C N W B Q Y H U X
Z G V W D N F E K O C I L M B T Q Y S J

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(19)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
K M T A X B P L E G F Q J Z Y V U H W N C S I O D R

Y D U X M Z L E J O S F V G K W B P R T H A Q I N C B S K F
T O E I L Q U H G J A R Y W N M C B X Z

(20)

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z
D P R A J T Q W K N Y C X V M I B Z F E O H S L G U

D A C O J R Q X G Y W B S P F I M V H N L K U E Z T M W F B
N Y X E Q U C L P G K H D I Z J T M O R

Correct transliteration of (20)

A D R M N Z B L Q G S P F I T K X H W V C Y O J U E X S T P
V G L J B O R C I Q Y W A K U N E X M Z

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THE MOMENTARY CHARACTER OF ORDINARY VISUAL STIMULI

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The fact that in rather common experience some distributions of brightness in the visual field are more acceptable than others has never found a physiological explanation satisfactory to the writer. Measurements of the foveal threshold, both for form and for brightness-difference, do not show, under the various experimental light conditions in question, differences at all commensurate with the differences in the varying degrees of comfort and discomfort expressed as the introspective findings of the users of lighting systems representing similar distributions.

The experimental results which the writer has in mind are from his own work of several years ago,[1] and consist in threshold measurements made in a bright rectangular field of small extent: 2.64 degrees horizontally and 1.95 degrees vertically. The photometric difference between the two halves of this field, necessary to a correct judgment of the direction of that difference, was taken as the threshold. The types of light distribution in the visual field were brought about by having this test field viewed in surroundings (or perhaps more clearly stated, upon a background) whose brightness could be independently and indefinitely varied.

Thus three typical conditions were possible, as well as all gradations between: (1) the condition in which field and surroundings were of equal brightness and which was treated as a standard; (2) the condition in which the surroundings were

[1] Cobb, P. W., The Effect on Foveal Vision of Bright Surroundings, iv. Jour. Exp. Psychol., i, 1916, pp. 540–66. Table V, p. 547 contains the data referred to. The preceding communications under like title are to be found in: Psychol. Rev., xx, pp. 425–47, ibid., xxi, pp. 23–32 and Jour. Exp. Psychol., i, pp. 419–25.

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brighter than the test field; (3) and the condition in which the field was the brighter. Practical light distributions of which these may be looked upon as extreme types might be said to be: (1) a well diffused and uniform illumination, presenting no more than moderate contrasts, (2) the conditions under which one attempts to look out into the night from a well-lit room, or attempts, in daylight, to see an interior through a window or open door, in such a way that the opening covers a comparatively small extent of the visual field and (3) a local, shaded light, close to the work, the rest of the space being in darkness.

Now as to the second of these conditions, it is to be said that a large increase in the difference threshold is to be noted when the surroundings are much in excess of the test field in brightness; the increase amounting to a large deviation from Weber's law. Visual acuity, under like condition behaves in a similar way. On the other hand it is to be said that such conditions are in no way a fair representation of any practical lighting conditions which have to be considered by the illuminating engineer as relevant to the comfort, efficiency or hygiene of the visual organs.

In the third case, that of a bright test field seen in dark surroundings, the writer's results show an increase in the value of the difference threshold, over its value under the standard mean conditions, of 55 to 70 per cent, according to the interpretation put upon the results. These figures are the means for three observers, and it is to be remembered that the experimental conditions are typical and extreme. The test field of less than three degrees angular extent is much smaller than any page or sheet ordinarily viewed at reading distance, and the surroundings, in the experimental arrangement, were as dark as technical devices could make them, which is not apt to be the case in the analogous practical situation.

Moreover the threshold is small (as large as 1 per cent of the test-field brightness in a single instance only), and the divergence of the values of the same, derived from separate series of judgments, is represented by a mean variation of 13 to 25 per cent of the mean. This degree of precision was adequate to make

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the comparison appear to be of physiological importance, but the method could not be proposed as a device for the technical appraisal of lighting installations.

Right here I wish to indulge in a small amount of discussion by way of criticism on the work just referred to. In it, the method of constant stimuli was used and the time of exposure was invariably three seconds, automatically controlled by the swing of a pendulum, and the observer was to consume all of that time, if necessary, in order to make his judgment.

If we reflect upon the performance of the eyes in ordinary use we will find that the instances in which the eyes remain in one position for a period such as three seconds, or even as much as a considerable fraction of one second, of time are either unusual, or they do not pertain to such situations as are implied in the phrase "use of the eyes"—or both. Instances in which the eyes remain stationary or nearly so for gross periods of time are:

1. Sleep, narcosis, coma and other states, medically known as states of unconsciousness.

2. The condition known as "brown study"—periods of abstraction in which one is caught, perhaps, "gazing into space," and in which, although the eyes appear to be in fixation, the object (if any) upon which they are fixed is, to say the least, remote from the center of attention.

3. The case of the observer in the laboratory where certain experiments in psychology and physiological optics are in progress which call for prolonged fixation. In such cases it is noteworthy that the brightness and color differences in objects tend to disappear, and that troublesome after-images are apt to take place on change of fixation.

4. Also, the somewhat unusual case of ordinary vision, including perhaps phases of certain special pursuits, when the object viewed is close to the threshold for the fovea and the observer gazes at it fixedly to arrive at a judgment.

The outstanding fact is, that in the "use of the eyes,"—in sewing, reading, checking and copying, and in "taking in" a drawing or a picture or the lay of a set of objects in space—the

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eyes are rarely more than momentarily at rest.[2] Certainly is this true of such work as is commonly accounted as "trying" to the eyes.

It is due to Troland,[3] starting from Hering's viewpoint, that the development of a new phase of the theory of vision has been forwarded, in which the change of state of the retinal elements is given first importance, and the conception of the (so to speak) one to one representation of stimulus-brightness in retinal response has been pushed into the background. Upon this hypothesis, granted that a stimulus of constant intensity continues to fall upon the same retinal area, the physiological condition in that area tends toward a certain state of equilibrium and approaches that state at a rate greater according to its remoteness from that state, somewhat after the manner in which the temperature of a body tends toward the temperature of the medium in which it either takes on or loses heat. Removed to a medium of different temperature, the body will at once tend toward the new temperature, by the same law, at a rate proportional to the temperature difference.

Similarly with the retina. The temperature of the medium represents that particular state of physico-chemical equilibrium in the retina which would hypothetically be induced by the continued action of the constant stimulus; a terminal state which the retina can, under constant conditions, be conceived to approach indefinitely without reaching it in any definite time. A new stimulus acts, and the retina begins a change of state, initially from whatever state it is in, toward a new terminal state; the initial change being the more rapid the more remote the new terminal state and *vice versa*.

[2] According to Huey (The Psychology and Pedagogy of Reading, New York, 1908) the movements executed by the eyes in reading occupy 0.04 to 0.06 second, while the average duration of the fixational pause he finds to be 0.19 second in one case, 0.11 in another. Dodge (Zeitsch. f. Psychol., lii, p. 373) states that the shortest adequate pauses photographically recorded in one of his experiments are 0.070 to 0.100 second.

[3] For a full presentation of this conception see: Troland, Am. Jour. Psychol., 1914, xxv, pp. 500–27; and further: Trans. Illum. Eng. Soc., New York, 1916, no. 9, pp. 947–66, and Jour. Opt. Soc. Am., i, 1917, pp. 3–15.

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But with the saltatory behavior of fixation, which has been well demonstrated by photographic records of the eye-movements executed in reading, no part of the retina—or at least not all parts critical in recognizing a given detail—can be conceived to reach a state of equilibrium, unless by way of a rare exception. The rule will be that a given retinal element only begins to make a change toward a certain state, when the process is cut short by a shift of fixation, before a state of equilibrium is even approximated.

Now what happens when, as in most experiments on visual thresholds, a part of the retina of a fixated eye is subjected to stimulation for one-half or one or several seconds? And what bearing has this on what happens to the retina in the case in which such a stimulus is cut off in a few thousandths or hundreths of a second? Obviously the two occurrences bear a certain mutual relation, but the latter represents the initiation of a change while the former represents more nearly the final stages of the same sort of change; the former is a type of stimulation whose result must be conceived to depend more largely upon the terminal state corresponding to the continued action of the stimulus, while the latter will give a result depending upon the quickness of the retina to respond, a result which, I believe, is more truly representative of the behavior of the sense-organ in the course of its every-day performance.

By way of thesis, for experimental verification or refutation, the following may be laid down:

1. Every-day vision involves, as a rule, only momentary fixation, during which most of the details of the total impression are taken in, not by the fovea, but by the parafoveal retina. Possibly the peripheral retina is best adapted to momentary stimulation. At least it may be said that the peripheral retina is highly sensitive to change of stimulus; and also to movement, which, when analysed as a stimulus, resolves itself into changes of stimulation of retinal elements.

2. The adequate stimuli to such vision are *a priori* by no means identical with those adequate to the foveal retina, especially when in the latter case the time of exposure to the stimulus

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is one of longer duration, as in the practical vision of a barely-visible object.

3. The condition of eye fatigue and eye strain are to be sought, not in relation to the unusual and more or less episodal performances of the eyes; but rather in relation to such performances of the visual organs as are the rule in the course of their ordinary use.

In detail, such performance of the eyes as is the rule in typical eye-work, as in reading for example, consists in successive momentary fixations, in which details of the words and of the letters forming them are impressed chiefly upon the parafoveal retina; and in sudden leaps from one point of fixation to the next, during which it seems probable that no neural impulse originating in an impression upon the retina is able to take part in the sensory integrative process.[4]

It is further to be noted in this connection, that the details passed over in these fixational leaps are by no means left out of the sensory integrative process. On the contrary, they appear to be the very details upon which the characteristics of one text depend, as distinguished from another. They are sensed as a result of momentary stimulation and they are sensed not through the foveal, but through the parafoveal retinal elements, which, even with a relatively leisurely mode of stimulation have been shown to be far behind the foveal elements in point of visual acuity. It would follow therefore that the stimuli which are adequate for the ordinary process of reading are, for the conditions under which they are seen, much nearer to the threshold than they would appear to be from a measurement of their visibility at the fovea made in the usual way with direct fixation and more than momentary stimulation; and it would seem, therefore, that a larger amount of our seeing is accomplished in fact at or near to the threshold than would at first thought be supposed.

[4] A conclusive piece of experimental work upon the retinal anesthesia which takes place during voluntary eye-movement is that of Holt, Psychol. Rev. Monog. Suppl., iv, pp. 3–47.

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Further, since the "exposure time" in ordinary vision is very brief, the coming to pass of adequate stimulation will depend upon the readiness with which the retina changes its state—will depend, in other words, upon the rapidity of the very first stage of the "rise of sensation" (or its fall) for that part of the retina involved and for the particular condition in which it happens to be at the moment. If that much be granted, it is not hard to see how the external conditions, antecedent and present, implied in the terms "adaption," or "contrast" and "glare" may induce a state of the retina which will affect the beginning of the response to a given stimulus; and make that particular duration of the fixational pause, to which the eye musculature is by idiosyncrasy and habit adjusted, inadequate for an otherwise supraliminal stimulus. A different rhythm of ocular movement must then take place; or else eye movements which are adequate under more favorable conditions will have to be repeated in order that successful integration of the retinal impressions may come about.

One or two informal observations, confirmed by the statements of persons other than the writer, may not be out of place here. The writer has noticed that certain technologically indefensible conditions of lighting may be perfectly comfortable as long as the eyes are not used for critical work. When reading is begun under the same conditions, discomfort referred to the eyes is felt at once or very soon. Again, eye discomfort sets in rather early when the material read is unfamiliar and hence difficult. Here the delay may be due to a lag in apperception. The fatigue incident to an unaccustomed rhythm of movement is well known in the case of one forcing himself to keep step in walking with another who, from anatomic, physiologic or temperamental causes has a step much different in length or period from one's own.

Theoretically, then, it would seem as though the momentary exposure method might throw light on certain questions related to the hygiene of vision.

A few remarks on the experimental aspect of this question will not be out of place. The first requisite is the control and

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measurement of extremely short times of exposure, say within the limits of the period of momentary fixation already established. The threshold may then be worked out either in terms of time, in terms of angular extent or in terms of intensity, the two other factors in any case remaining constant. The classic rotating disc seen by reflected light will probably have to be discarded, as with it the simultaneous control of the time of exposure and of general contrast and adaptation factors is not practicable.

A further suggestion is the reduction of the extent of the stimulus to that of a "physiological" point, making it the same order of quantity as a star-magnitude, photometrically expressible simply in terms of illumination upon the eye.[5] It is interesting to speculate as to whether experimentation will show that by this method the threshold may be consistently expressed, without further specification as to size of stimulus field, in a single numeric, having the two dimensions, flux and time.[5] There is some evidence[6] to make this plausible, and such relation, if it can be established, will be a great help in the standardization of method and in the intercomparison of results in visual threshold work.

[5] If the brightness of the stimulus be b, and its area s, its candle-power will be bs, and the illumination (due to the stimulus) upon the eye at distance r will be bs/r^2. The pupillary area has not yet been taken into account. Its area, a, determines the light-flux which enters the eye and goes to form the image of the stimulus. This flux will be abs/r^2.

[6] Rivers (Schäfer's Text-book of Physiology, vol. II, p. 1067) says: ". . . . according to Charpentier, for a given sensation a certain amount of light is necessary, and within the limits it matters little whether this amount is extended over a larger or smaller area, or reaches the eye in a longer or shorter time." The limits referred to are: as to time, 0.002 to 0.125 seconds; as to spatial extent, the upper limit has been described as "corresponding approximately to the dimensions of the fovea." (p. 1055). The quantitative statements are atrtibuted to Charpentier, La lumiere et les couleurs, Paris, 1888, p. 138, and Arch. d'opht., Paris, 1890, tome x, p. 108.

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RELATIVE DISTANCE AS A FACTOR IN THE WHITE RAT'S SELECTION OF A PATH

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Elimination of errors and decrease in length of path from starting point to goal (usually food) are characteristic of animal learning. Of two paths leading to food, one being longer, the animal soon chooses the shorter. This has been observed with white rats in their learning of mazes. This article is a report of an attempt to study this selection by the white rat of the shorter of two paths.[1]

Two mazes, one rectangular and one circular, were used. The rectangular maze will be referred to as maze A; the circular, as maze B. Maze A (see diagram 1) was constructed of 12-inch redwood boards placed edgewise on the floor. Dimensions of maze A, 14.2 by 7.2 feet, outside measurement. Width of path, 1 foot. There were two food-boxes, F_1 and F_2, and one starting box, H. The starting box was fixed at the center of one of the shorter sides, and the rat was admitted into the maze by means of a sliding door and string. The maze was covered with galvanized wire mesh. Length of entire path, 38.8 feet. Maze B (see diagram 2) was circular. Diameter, outside to outside, 6 feet. Path, 6 inches wide and 34.6 feet long. The walls were made of black cardboard strips fastened to the floor. Height of walls, 8 inches. Starting box H, fixed. Food-boxes, F_1 and F_2, were adjustable for different distances from H. S, F_1, and F_2 all had doors controlled by strings from the observer's station.

The method was the same for both mazes. One of the food-boxes was provided with food and placed at a certain distance

[1] The experimental work reported in this article was done in the psychological laboratory of Stanford University. Its incompleteness is apparent. The writer's transfer from Stanford to Pennsylvania State College occurred before the work could be completed.

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from *H*. The door of the box thus provided was kept open until the rat reached the food. The other food-box was located at an equal distance from *H*, but on the opposite side; the door of this box was kept closed. The rat was put into the starting

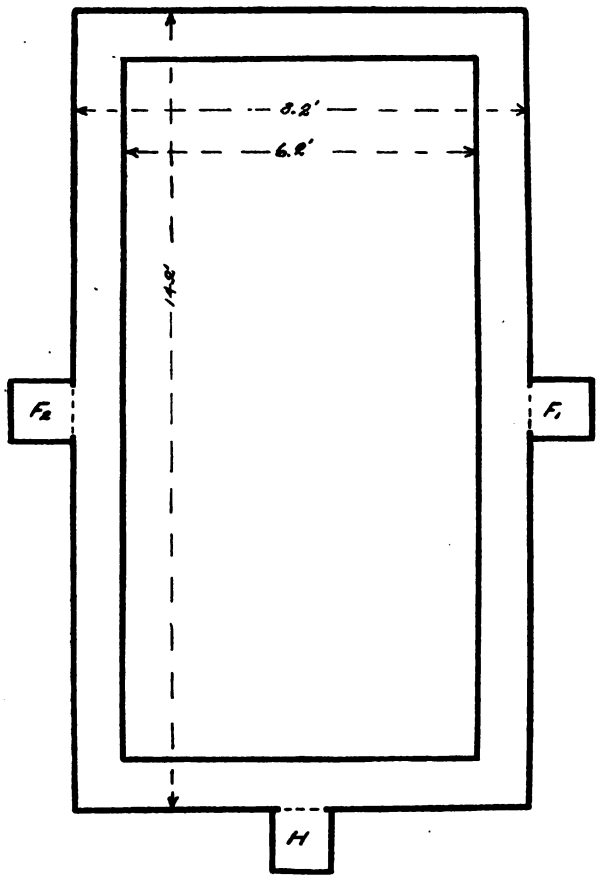

DIAGRAM 1

box and admitted into the maze by opening the door of *H*. At first the food-box on the right (or left) contained food until the rat through repeated trials learned to go the shorter way, then the food was changed to the other box and the door of that box kept open, the door of the food-box previously used

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being closed. The door of only one food-box was open at any one time. By alternating the position of the food-boxes the influence of any tendency to form an association between the food and direction of turn was eliminated. At the beginning of a series of experiments with any rat the difference between

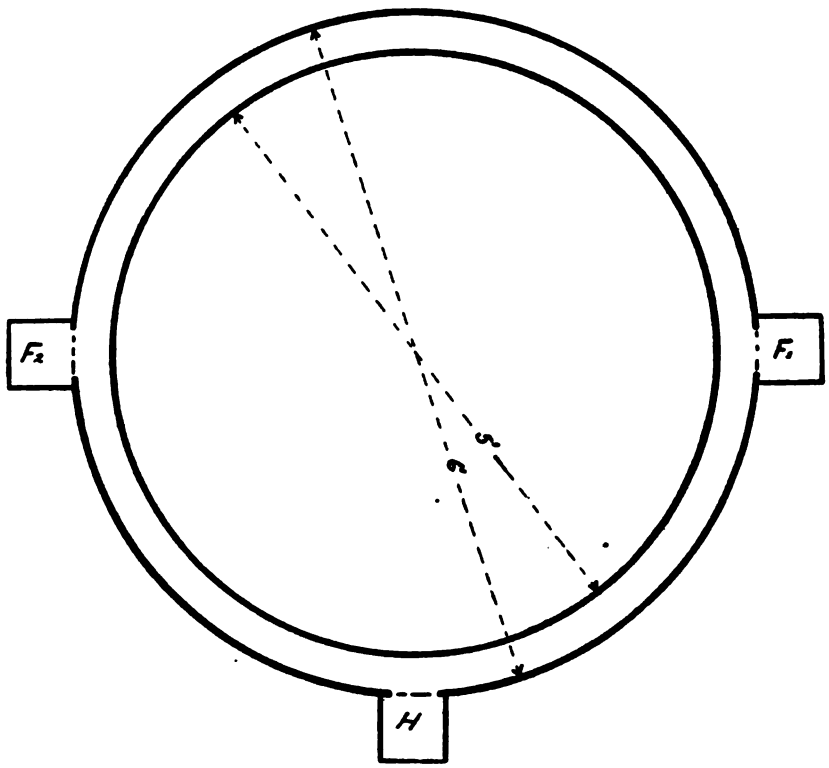

DIAGRAM 2

the short and long path was relatively large and was made smaller and smaller until the rat after repeated trials failed to select the short path. Usually 10 or 15 trials were made on each experimental day with the direction and distance of the food-box constant. In no case was the food-box visible from the rat's starting point.

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Six white rats were used, four males and two females.

Rat 1: Male, about six months of age
Rat 2: Female, about six months of age
Rat 3: Male, about two months of age
Rat 4: Male, about two months of age
Rat 5: Female, about two months of age
Rat 6: Male, about two months of age

The following explanation holds for all tables. Under "days" (first column) is given the number of experimental days required for the animal to adjust itself to the shorter path, i.e., until the rat was clearly taking (preferring) the shorter path. Under *S* and *L* (second and third columns) appears the absolute number of times in the given experimental days the rat took the shorter (under *S*) and longer (under *L*) paths. It may be pointed out here that if the rat has come to find food on the right side, for example, and has established a habit of going in that direction, the sudden shift of the food by the experimenter to the opposite side (left in our example) is not followed immediately by a tendency to go in the opposite direction, but the old direction is taken at first and then less and less often, with increasing number of trials, until the number of times he takes the new direction during the day's experiment begins to exceed the times he takes the old. Obviously this shows adjustment, and to indicate it *S* need not be greater than *L*, but the experimenter usually continued the trials until adjustment was quite certain. Under Dn (fourth column) is given the direction from *H* of the box containing food (indicating the shorter path) for that particular set of trials: *R*, right; *L*, left. Under R (fifth column) is given the ratio of the length of the long to the length of the short path. Now if

P_t = total length of path
P_s = length of short path
P_l = length of long path
D = absolute difference between long and short paths
R = ratio of length of long (P_l) to length of short (P_s) path

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then it is readily seen that P_s, P_l, and D can be evaluated from the data given in the tables by the following,

$$P_s = \frac{P_t}{1+R} \tag{1}$$

$$P_l = P_t - P_s = \frac{P_t R}{1+R} \tag{2}$$

$$D = P_l - P_s = \frac{P_t(R-1)}{1+R} \tag{3}$$

It should be remembered that in the case of maze A P_t = 38.8 feet, and in case of maze B P_t = 34.6 feet.

Experiment 1. Maze A; rat 1. The results are presented in table 1.

TABLE 1

DAYS	S	L	Dn	R
1	9	1	R	3.02
1	9	1	L	3.02
2	21	4	R	1.95
2	17	10	L	1.95
1	13	2	R	1.95
2	22	8	L	1.33
1	11	4	R	1.33
2	18	12	L	1.23
4	34	26	R	1.23
6	41	49	L	1.17
6	47	43	R	1.17
4	34	26	L	1.11
2	24	6	R	1.11
5	41	27	L	1.05
8	10	110	R	1.05

In this experiment rat 1 shows preference for the shorter path when the longer path is 11/100 longer than the shorter, and adapts to a difference of 5/100 in one direction, but fails in the other direction.

Experiment 2. Maze A; rat 2. Table 2 gives the results.

Rat 2 shows ability to select the shorter of two paths when the longer exceeds the shorter by one-twentieth of its length.

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TABLE 2

DAYS	S	L	Dn	R
6	26	3	R	6.5
2	9	1	L	6.5
1	8	0	R	2.92
1	8	2	L	2.92
2	13	7	R	1.95
2	16	4	L	1.95
2	15	5	R	1.95
1	8	2	L	1.40
1	7	3	R	1.40
1	8	2	L	1.26
2	12	8	R	1.26
7	49	56	L	1.17
1	12	3	R	1.17
2	21	9	L	1.17
7	63	42	R	1.11
2	21	9	L	1.11
8	61	59	R	1.05
8	71	49	L	1.05

TABLE 3

DAYS	S	L	Dn	R
2	9	2	L	2.72
1	9	1	R	2.72
1	7	3	L	1.59
4	35	25	R	1.59
1	12	3	L	1.59
2	20	10	R	1.32
1	14	1	L	1.19
3	25	20	R	1.19
1	13	2	L	1.12
7	53	52	R	1.12
2	20	10	L	1.06
13	33	162	R	1.06
5	5	45	R	1.12
4	2	38	R	1.19
1	0	10	R	1.32
1	0	10	R	1.59
1	7	3	R	2.11

Experiment 3. Maze B; rat 3. Results appear in table 3. The fraction of noticeable difference in the case of rat 3 is 12/100 in either direction and 6/100 in one direction, but failure in the

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other. The influence of habitual action is clearly seen in that the relative difference between long and short paths could be rapidly increased without adjustment until the ratio of long to short path was 2.11. Since the rat had previously as small a fraction of noticeable difference as 12/100, additional trials, after failure at 6/100, doubtless would again have led to adjustment at 12/100. By gradually increasing the relative difference between the lengths of the two paths and limiting the number of trials habitual action rather than length of path seemed to determine the rat's course.

Experiment 4. Maze B; rat 4.

TABLE 4

DATE	*S*	*L*	*Dn*	*P*
5	23	1	R	2.72
2	17	3	L	2.72
2	14	6	R	1.59
2	11	9	L	1.59
4	29	31	R	1.32
1	11	4	L	1.32
2	19	11	R	1.19
1	10	5	L	1.19
10	78	72	R	1.12
1	12	3	L	1.12
15	63	162	R	1.06
12	57	63	R	1.12
1	8	2	R	1.06
1	5	5	L	1.12

This rat gives a fraction of noticeable difference equal to 12/100 and failure at 6/100. The adjustment to 12/100 is further confirmed by the fact that after failure at 6/100 an increase to 12/100 again brought out the tendency to take the shorter path, and was sufficiently effective to break the rat's habitual tendency towards the left and bring about a change to the right. Again, a shift of food-box from right (after adaptation) to left is followed by an immediate tendency towards adjustment at 12/100.

Experiment 5. Maze B; rat 5.

Adjustment to 12/100 and failure to adjust (in the trials given) to 6/100 is shown.

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TABLE 5

DAYS	S	L	Dn	R
6	29	13	L	2.72
2	15	5	R	2.72
1	8	2	L	1.59
7	27	43	R	1.19
2	16	4	L	1.12
20	73	127	R	1.12
4	15	25	L	1.06

Experiment 6. Maze B; rat 6.

TABLE 6

DAYS	S	L	Dn	R
3	20	1	R	2.72
3	20	10	L	2.72
2	12	8	R	1.59
2	13	7	L	1.59
3	15	15	R	1.32
5	30	20	L	1.32
4	20	20	R	1.19
4	11	29	L	1.19
4	26	14	R	1.12
16	61	99	L	1.12
1	7	3	L	1.19
1	8	3	R	1.19

Adjustment to 19/100 in both directions and 12/100 in one direction, but failure in the other. Adjustment to 19/100 is further confirmed.

Experiment 7. Maze B; rat 1.

TABLE 7

DAYS	S	L	Dn	R
1	10	0	L	1.19
18	83	97	R	1.19
5	30	20	L	1.19
2	12	8	R	1.12

Adjustment is shown to 12/100 in one direction, in a small number of trials.

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Experiment 8. Maze B; rat 2.

TABLE 8

DAYS	*S*	*L*	*Dn*	*R*
9	37	53	R	1.19

Adjustment to 19/100 in one direction is shown.[2]

It is clearly shown that the length of path is a factor that may influence the rat's course. Other things equal, the shorter of two paths will be traversed more and more until it becomes the usual one, provided a certain fractional difference in length exists between short and long paths. Our results indicate that this fractional difference is as low as 1/10, and in some cases lower. This means that of two paths leading to the same goal one of which is n feet long and the other $1.1n$ feet, after repeated trials, the former will be generally traveled, and it will be thus favored because it is *shorter* than the other. It may be conjectured that this adjustment is to be explained in motor terms. A number of cases appeared throughout the experiments of the rat's making a start in the wrong direction and suddenly changing about and taking the right direction, resembling, in outward appearances at least, a kind of behavior occasionally seen in human beings.

[2] In this case as well as in one or two others the writer's statement may not appear justified by the figures given under *S* and *L*, however it is based upon an examination of the detailed results. For example, the results of the nine experimental days given in table 8 were distributed thus,

DAY NUMBER	*S*	*L*	*R*
1	2	8	
2	2	8	
3	4	6	
4	2	8	
5	5	5	1.19
6	3	7	
7	6	4	
8	6	4	
9	7	3	

The shift from long to short path is obvious.

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INTERRELATION BETWEEN HABIT FORMATION AND FEELING QUALITIES OF VOLUNTARY MOVEMENTS[1]

LINUS WARD KLINE
Skidmore College

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[1] The many obligations incurred during the course of this investigation cannot be adequately indicated here. I can but suggest the sources of my indebtedness.

To Harvard University I am grateful for the privileges and benefits of a Research Fellowship in Psychology for the scholastic year of 1912–1913, and to the late Prof. Hugo Münsterberg for his counsel and encouragement throughout the work. My hearty thanks are hereby expressed to Prof. Herbert S. Langfeld for supervising the preparation of the apparatus and for many painstaking criticisms and helpful suggestions. For the patient service of those who acted as subjects throughout the year under tedious and exacting conditions, I am deeply indebted. All credit is due my wife for the arrangement of the material and for the preparation of tables. It is also a pleasure to acknowledge the valuable suggestions given by Prof. Knight Dunlap during the revision of the original manuscript.

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INTRODUCTION

The psychology of the new century is seeking to discover how the mind performs its task in the work-a-day world. The pioneer study in this field was made by Bryan and Harter (7) on learning telegraphic language, and it marked the beginning of a line of studies of the mental processes involved in performing industrial work of high commercial value. Years later Book (8) made a careful study of the art of typewriting, Diehl (12), Downey (13), Judd and others of the hand writing process; and, similarly, Münsterberg (31) has studied the complex mental operations involved in ship-navigation, in running an electric car and in

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manipulating a telephonic switch-board. Along with and prior to these researches considerable work had been done on inhibition, interference and transference, setting forth the mutual effects of such interaction. These investigations have ranged from the determination of the conditions for inhibiting the croak in the frog to those governing the inhibiting and facilitating functions of associations, judgments and memories of the human mind.

These two groups of investigations, while recognizing the influence of feeling tone on the learning functions involved, attempt nothing further than irregular and incomplete references to such phenomena. Swift (37) observed that the feeling tone of confidence and the bodily tone of "off days" affected the number of catches in ball tossing; Cleveland (9) found that young and ardent chess players, especially, testify that the elation of victory and the bitterness of defeat are factors to be considered in learning the game. Acquiring skill in typewriting, according to Book, shows "marked fluctuations in feeling and attitude . . . . . from moment to moment of every test from day to day and at different stages of practice." "A high degree of spontaneous attention was always marked by a pleasureable feeling-tone; a failure in it by feelings of displeasure or disgust." "The rôle of feeling in successful learning is perhaps not so clear and deserves a word of comment. Two questions naturally arise. (1) What is the meaning of the perfect correlation between the fluctuation in attention and effort . . . . . and changes in feeling and attitude? (2) What retroactive effect have these fluctuations in attitude and feeling upon the variations in attention and effort that went with them?" and he further points out: "It by no means follows that the relation between these changes in feeling and attitude and the fluctuations in attention and effort is casual because the parallelism is perfect. Whether the feelings and mental attitude of the learners give rise to the fluctuations in attention and effort and the consequent success or failure in writing, or whether the fluctuations in attention and effort cause the changes in feeling, or whether both are joint effects of a still more fundamental cause, is a matter with regard to which it is impossible to make a general statement."

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This quotation from Book introduces the general field of the present investigation. Further orientation is secured by making brief reference to a long line of investigations looking to the determination of the nature of the relation between movements and their accompanying feeling states. The earlier studies were of the physio-graphic (expressive) type placed on a scientific basis by Mosso (28). The methods, inventions and adaptations of technique growing out of the original work by Mosso had practically, by the end of the nineteenth century, explored all those relationships existing between the vital activities of the body and their concomitant feelings. Coincident with such studies were those involving the relationship between involuntary movements and mental states. The researches of Jastrow (21) of Beard (2) of Downey (13) and of several others reveal surprisingly minute and orderly interplay between both ideational and affective states on the one hand and involuntary movements on the other; so constant are the several relationships, that their modes of expression, to the trained observer, serve as reliable indices to the general character of the mental content. These delicate relationships between affective states and movements attain their greatest complexity and significance when movement is voluntary. These studies, beginning with Féré (15) have received important contributions from Münsterberg (32), Störring (39), Woodworth (43), Rose (35) and many others. They agree in showing that accuracy of movement, as well as speed, is affected by the presence of feeling states. Münsterberg found a correlation between a constant negative error in movement and bodily languor, and between a constant positive error of movement and bodily vivacity. Seriousness gave a negative constant error; gaiety a positive one. Pleasure gave a positive constant error for extension of the arm, and a negative constant error for flexion; whereas unpleasant feelings gave excessive flexions, and negative errors of extension. Rose shows that the latent period of simple reaction time is reduced with the increase of unpleasant stimulus, that is, latent period and increase of stimulus are inversely proportional; and also that unpleasant feeling, from whatever source, from weak to strong intensity, increases the motor strength.

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Recent investigations have been made to determine the effect of feeling tone on the memory processes, the results of which might be more conclusive.

But the literature, so far as I know, records no attempt to determine the effect of the feelings reflexly generated by a voluntary movement upon the movement's value as a factor in skillful action. Book's incidental observations are the nearest approach to the problem.

PROBLEM

Accordingly, the present investigation seeks to determine the nature and extent of the influence of feeling qualities of movements on their entering into skilful combinations. Here a number of questions may be raised: (1) Does the unpleasant feeling growing out of a movement *per se* inhibit or facilitate its entering into a combination of skilful acts? (2) Does a pleasant, or a tense, or any other feeling quality, generated by the coördination of any group of muscles, affect their habit-forming values? or (3) Is there no causal relation between the capacity of a movement for skilful purposes and its feeling quality of whatever sort? (4) What, if any, are the psycho-technical values of such a study?

METHODS AND APPARATUS

The above statement of the problem foreshadows, in a measure, the group of methods essential for such an investigation. It appears that the method should involve learning in which the acquisition of motor skill may be registered in many degrees of improvement, and in which varied feelings, for the most part, are constant accompaniments. Accordingly, a method was devised whereby both objective and introspective records were obtained for every phase and stage of the learning process. Previous experience in the use of a distributing case (diagram 1) and packs of ordinary playing cards for a study of interference and transference (Kline and Owens 25), suggested the possibility of adapting them to the present problem. Jastrow (20) in 1886 first made use of the ordinary playing cards in determining the

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several forms of reaction time. Bergström (3) in his memory experiments has used cards marked by words and letters. Kline and Kellogg (26) have described at some length the many advantages of cards, and especially playing cards, for psychological apparatus. Since completing the present research, there has come to the author's notice the paper of Professor Washburn (42) on the speed of motor learning, and Culler's (11) card sort-

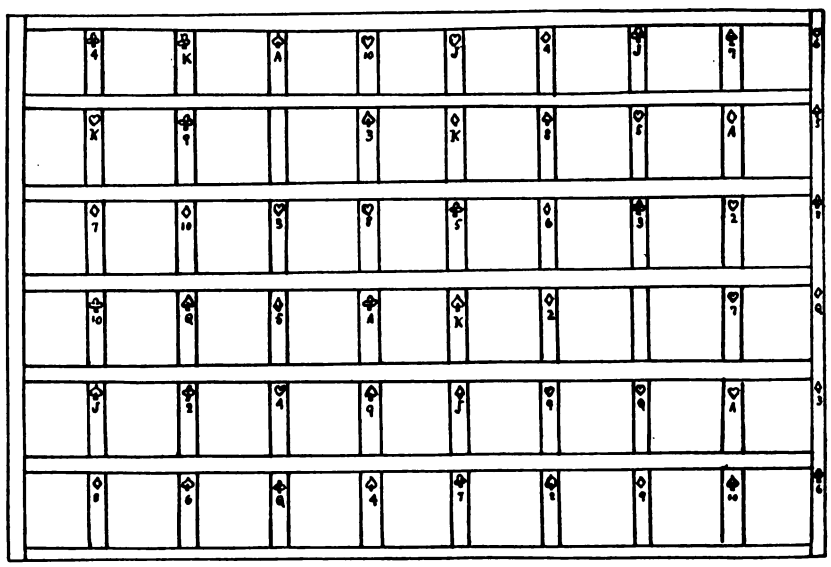

DIAGRAM 1. Showing the arrangement of the boxes in the Distribution Case. Plan I—S to D, D, to C, to C, H, to H. To be read: Spades are thrown to diamond boxes, diamonds to club boxes, clubs to club boxes, and hearts to heart boxes.

ing experiment in his study of Interference and Adaptability. The latter used "Flinch" cards. In neither case were the cards stacked in a constant order.

A distributing case 3 feet by 4 feet 8 inches was constructed out of 54 individual boxes, having 6 in the vertical dimension and 9 in the horizontal. This arrangement furnished a box for each card of the pack of 52, and at the same time preserved an ap-

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proach to equality between the dimensions without a large excess of boxes. The boxes were 3.5 inches in width, 2 in height and 4 in depth, thus permitting a free unhampered distribution by a slight tossing of the card, if the subject so desired. Uniformity of labels for the boxes was secured by using the picture labels printed on the corners of the playing cards. These were inserted in clips at the upper right corners of the boxes (diagram 1). Prominence was given to the labels by staining the boxes a dead black. The lower edge of the bottom row of boxes and the lower edge of the upper row was 37 and 54 inches respectively from the floor. A black cardboard fixed to a wooden frame, supported by a pulley and adjusted to move up and down readily in front of the case served to expose or screen the case as required. Two cases were fitted up for two slightly different plans of work and placed in a dark room. The electric lamps used for lighting bore reflectors so adjusted as to throw the light on the case and at the same time to shield the eyes of the subject from its glare. A small space, 4 by 5 feet was curtained off for each case. This enabled the subjects to work unobserved and afforded conditions quite similar to those employed in railway mail cars and in central post offices for distributing mail.[2] Over and above the feelings growing out of the distributing process, which were likely to appear, wax and wane irregularly in time, quality and intensity, it seemed desirable to create feelings by artificial means with the view to control their temporal and qualitative ways of appearing. For this purpose supposedly pleasant and unpleasant visual, auditory, olfactory and gustatory stimuli were employed. The visual stimuli consisted of artistic pictures, and of photographs of the human body bearing malignant tumors and surgical scars; the pleasant auditory was made by three harmonious tuning forks mounted on resonant boxes, and the unpleasant auditory, by a half dozen empty cocoa cans tied in a

[2] Some fourteen years ago I observed two men as railway mail clerks distribute "practice" cards to a mail case labeled with routes for dispatching mail. The combination of cards and practice-case suggested the possibilities of their use as apparatus for the study of the integration of movements and of the development of the sense of position.

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bunch and dragged about the floor and knocked against objects in the room; for unpleasant taste, quassia wood was used, and for pleasant, candied ginger, chocolate and fruit candy, salted peanuts, etc.; the unpleasant odors consisted of pyridine and asafoetida; the pleasant were the usual perfumes of the toilet, such as violet, white rose, heliotrope, etc. The subject's favorite tastes and odors were used whenever possible.

The method, then, consisted in an application of the earning process under controlled conditions to a study of the influence of feeling of voluntary movements on their integration and coördination into skilful action.

PLANS AND PROCEDURE

Distributing 52 cards, one at a time, requires as many consecutive movements. By always stacking the cards in the same way and by distributing them under the same plan to the same fixed boxes, the 52 movements became roughly divided into 8 systems consisting of the 4 cardinal and the 4 semi-cardinal points of direction, viz., up, oblique right up, right, oblique right down, etc. Furthermore since the sequence of suits is repeated 13 times in the pack and the numerical order four times, it is possible to classify the 52 movements into 4 sets ending, respectively, with the thirteenth, the twenty-sixth, the thirty-ninth and the fifty-second card. These sets are referred to as the *first*, *second*, *third* and *fourth* 13, and are represented in diagrams 2a and 2b. A glance at first 13 in plan I shows the prevailing direction of the movements for that set to be oblique up right and oblique left down.

Experimentation began October 14, 1912, and continued with one hour period a week for each subject, without interruption save for the usual vacations, until the middle of May 1913. Table 2 gives exact dates, extent of time, etc., covered by the experiment.

Eleven subjects, all members of the Harvard Graduate school save one, and trained psychologists, served throughout the year. Nine members, A, B, C, E, F, G, H, I, and J used plan I. This

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required that spades be thrown to diamond boxes, diamonds to club boxes, and clubs and hearts each be distributed to boxes bearing their own labels, i.e., clubs and hearts were "resident," each card being thrown to a box bearing its number. Since spade boxes were not used, only 39 of the 52 labelled boxes were involved in the investigation. The order of the suit was: clubs, diamonds, spades, hearts (referred to as C, D, S, H); and the numerical order was: Q, 5, 6, 4, 3, 10, 8, 2, K, 9, 7, J, A. Two subjects, D and K distributed C to H, and H to S, and S and D to their respective boxes—using only 39 boxes of the case. The order of the suit was identical with that of plan I, but the numerical order ran 10, 8, 6, Q, 5, K, 7, A, J, 9, 4, 2, 3, This formed plan II. Subjects A, B, and C in plan I, and D in plan II, were given pleasant stimuli; subjects E, F, G, and H, plan I, received unpleasant stimuli, while subjects I and J plan I and K, plan II, worked under normal conditions. The 8 stimuli subjects were given their respective stimuli at every other period, beginning with the third; *normal periods* then, alternated with *stimuli periods*, the latter always occurring on the odd number. Five distributions were made by all subjects at each period (one hour) except with the first six periods. At the first period, owing to the novelty of the situation and to the difficulty of grasping the instructions, only from one to two distributions were made. The number gradually increased up to the sixth period, when it became evident that all subjects were able to make five records per period (one hour). The amount of practice permitted during the first six periods was governed by the progress of the slowest subjects. The stimuli applied to the four senses, while not always given in the same order, were presented in a cumulative fashion, e.g., before the first distribution, odors and tastes; before the second, odors, tastes and sound; before the third, odors, tastes, sounds and pictures: and all four kinds were given before the fourth and fifth trials. A trial consisted in delivering the pack of 52 cards to the boxes according to the plan already indicated. A distribution comprised five phases: (1) the subject was given the stimulus; (2) he took the deck of cards as in the act of distribution and stood directly in front of the case; (3) the signal

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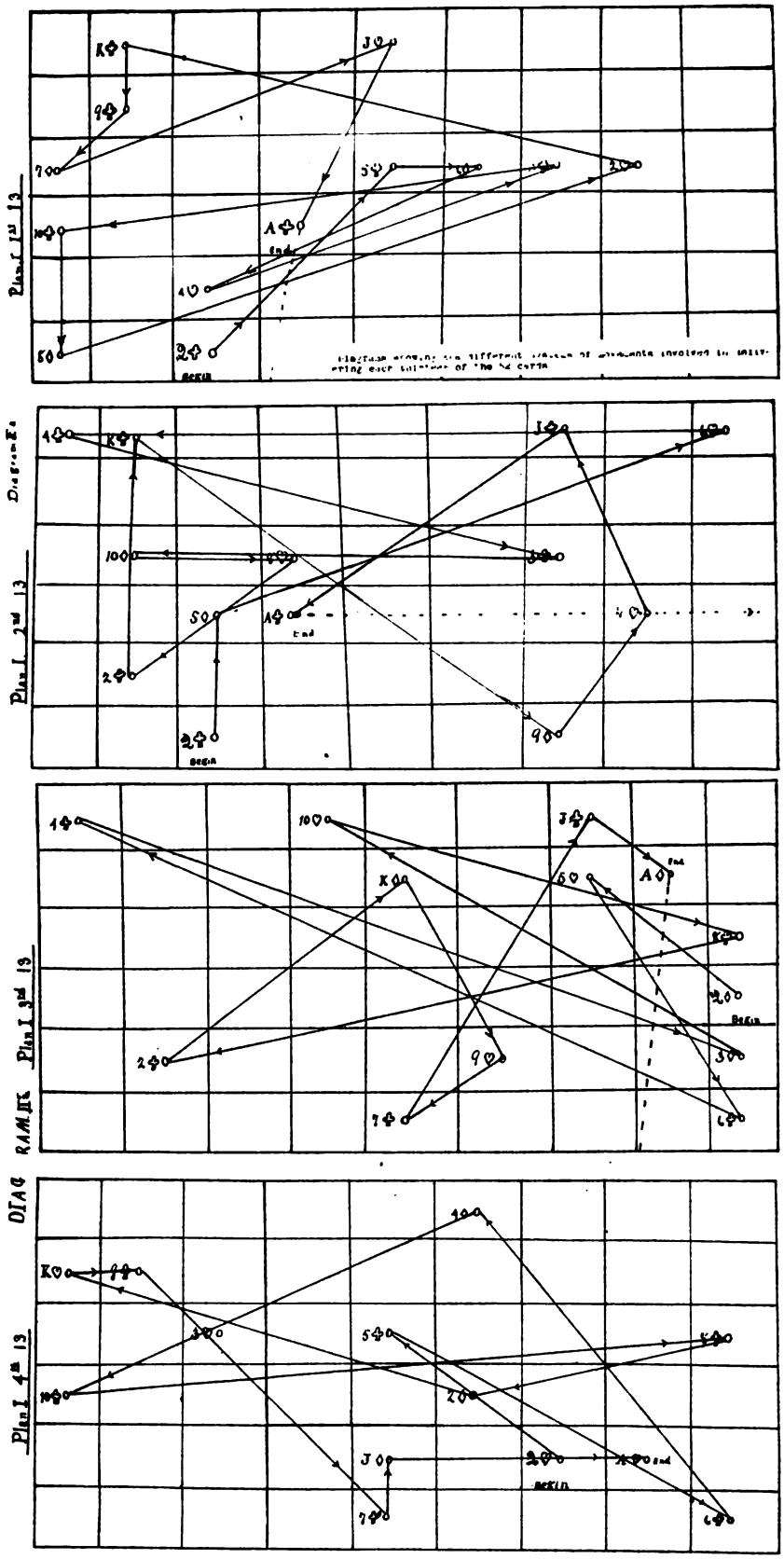

DIAGRAM 2. (a) Plan I—Showing the different systems of movements involved in delivering each 13 of the 52 cards. (b) Plan II—Showing the different systems of movements involved in delivering each 13 of the 52 cards.

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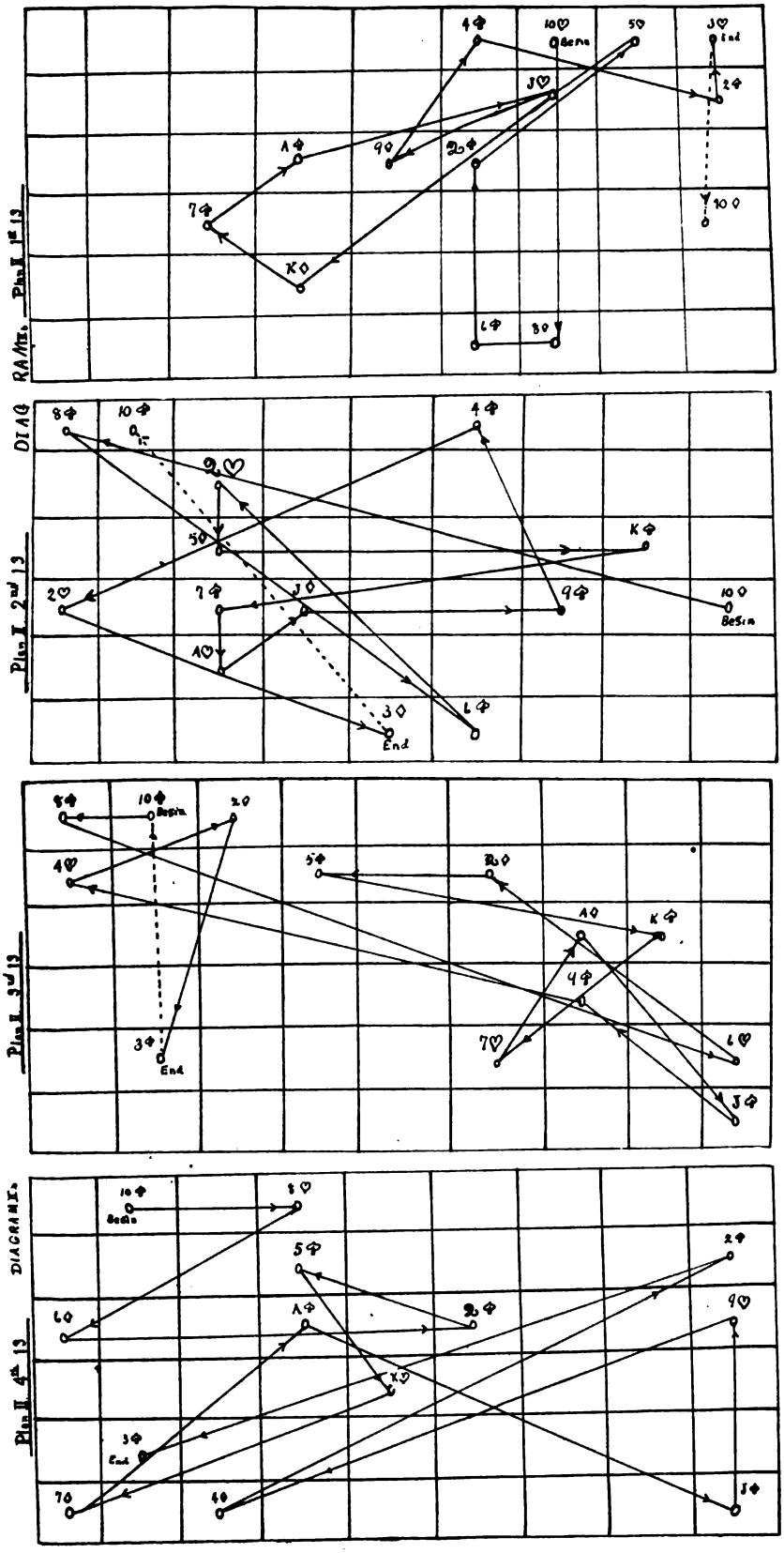

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"ready" was given, a moment later the screen lifted clear of the case and at the same time a stop watch was started and the subject began the distribution; (4) as the last card was thrown the subject called "up," as a signal to stop the watch and drop the screen; the time between raising the screen and the subject's signal "up" was considered the "distribution time;" (5) the rate of distribution and the subject's introspections were recorded. As the learning advanced, it became necessary to record errors and the kind and number of cards that noticeably checked the distribution. These latter inhibiting cards are referred to as "hold-ups" and are so tabulated.

Supplementary experimentation and observation will be described in connection with the results which they are designed to elucidate.

CLASSIFICATION OF RESULTS

A. General results

The results of the main problem are arranged, discussed and compared according as the subject worked under pleasant, unpleasant, or normal conditions. On this basis the results consist: (1) of the distribution rates expressed in "learning curves;" (2) of the subject's introspections; (3) of the relations between the rate of learning, the location of the boxes and the rate of distributing the cards; (4) of the growth of the synthetic processes with increase of speed; and finally (5), of errors, of dropped cards and of hold-ups in relation to stimulus and to normal conditions.

B. Supplementary results

As the general results accumulated, additional experimentation appeared necessary to support, check and explain them. These subsidiary results consisted: (1) of "memory holds up" secured by requiring the subject to recall in due sequence the 52 movements. Here the degree of correlation is sought between the remembered and unremembered movements on the one hand and their feeling qualities on the other. (2) Of both a "memory" and a "perceptual" report of the feeling qualities of each of the

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52 movements. Interest here is in the possible correlation between the reported feeling qualities and the direction of the movements together with the 'memory" hold-ups and hold-ups occurring in *actual* distribution, errors and dropped cards.

The "memory" reports of the affective tone of the movements were taken a week before getting the report of feelings as they occurred with movements in "a series" and in "isolation," i.e., as an individual movement. These latter reports, described later, were continued from eight to nine weeks. The same validity is attached to these 'reports' of feelings as facts, that is ordinarily given to one's report of a headache or to the sting of a corn, no more nor less.

In making the "memory report" of feelings the subject sat at a table and with a chart of the movements before him indicated their feeling qualities in terms of pleasant (+), unpleasant (−), neutral (0), tense (t) and relaxed (r): the instances of relaxed feelings were so few that they are not reported in the tables. Only seven subjects of plan I knew the movements well enough at the twelfth hour of practice to make reliable reports. Subjects B and H were unable to make any from memory.

The reports of the feeling quality of a movement as it was being executed—"perceptual report" as contrasted to "memory report" —were of two kinds:

1. The subject was required to report on the feeling quality of a movement, using the terms required in the "memory report" (+), (−), etc., regarded in its relations to the system or set of movements in which it occurred in the distribution, i.e., in relation to the movements that preceded and immediately followed it.

2. To report on its feeling quality regarded as an individual movement made between two boxes of the case and as if it were the only movement made. The former reports are tabulated under the heading "A series" and the latter under that of "isolation." (see table 3). The "serial" report of the entire 52 movements was made first, and after a few minutes pause, the "isolated" were taken. In both I called the names of the cards in the stacked order and the subject standing before the case in

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TABLE 1

Showing records for distributing pack of playing cards to thirty-nine compartments

NUMBER EXPERIMENT	STIMULUS																					
	Pleasant								Unpleasant								Normal					
	A		B		C		D		E		F		G		H		I		J		K	
	Rate	Percentage of increase	Rate	Percentage of increase	Rate	Percentage of increase	Rate	Percentage of increase	Rate	Percentage of increase	Rate	Percentage of increase	Rate	Percentage of increase	Rate	Percentage of increase	Rate	Percentage of increase	Rate	Percentage of increase	Rate	Percentage of increase
	seconds		seconds		seconds		seconds		seconds		seconds		seconds		seconds		seconds		seconds		seconds	
1	15.66		10.83		16.34		15.23		11.06		12.65		13.15		18.65		12.58		11.74		13.08	
2	10.26	34.36	10.06	7.12	12.88	21.17	11.87	22.23	8.29	25.00	10.04	20.65	7.91	39.86	16.21	34.65	8.64	31.30	8.65	26.35	11.66	10.88
3	9.44	40.97	9.07	16.33	12.07	26.14	11.15	26.95	8.29	25.00	8.22	35.06	7.32	44.32	12.79	47.92	7.80	37.96	6.74	42.59	10.19	22.09
4	6.49	58.56	7.33	21.68	7.88	51.80	9.93	37.47	5.99	45.83	5.38	57.45	4.86	63.07	10.79	57.19	4.54	63.91	6.24	45.20	8.26	36.85
5	4.66	70.25	6.13	43.47	4.89	70.56	6.99	54.25	4.35	60.67	4.37	65.44	3.53	68.71	9.61	61.28	3.16	74.84	4.40	62.55	7.03	46.21
6	2.64	83.10	6.19	42.87	3.29	79.88	7.00	53.64	3.37	69.56	2.90	77.16	2.44	81.46	9.13	63.22	2.43	80.63	3.36	71.34	5.02	61.63
7	2.12	86.46	5.01	53.72	2.95	81.92	4.86	68.53	2.66	75.94	1.94	84.63	1.93	85.34	7.51	69.74	1.74	86.15	2.20	80.13	3.62	72.35
8	2.07	86.84	4.03	62.84	2.69	83.54	3.89	74.53	1.92	80.92	1.57	87.58	1.70	87.08	6.61	73.30	1.64	86.88	2.03	82.67	3.53	73.03
9	1.99	88.73	3.66	66.19	2.11	87.11	3.34	78.13	1.42	87.18	1.38	89.06	1.50	88.58	5.24	78.86	1.28	89.79	1.63	86.24	2.55	80.53
10	1.36	91.29	3.25	69.99	2.03	87.55	2.92	80.86	1.06	90.38	1.21	90.47	1.35	89.75	4.53	81.73	1.12	91.09	1.89	83.88	2.25	82.92
11	1.38	91.01	2.64	72.20	1.88	88.51	2.32	84.77	0.98	91.18	1.21	90.43	1.31	90.06	4.76	80.77	1.05	91.62	1.44	87.73	1.79	87.79
12	1.34	91.45	2.12	80.40	1.77	89.15	2.69	82.42	0.96	91.31	1.03	91.83	1.18	91.01	3.01	87.83	0.99	92.08	1.55	86.16	1.46	88.88
13	1.20	92.34	1.97	81.77	1.66	89.83	2.31	84.83	0.87	92.18	1.05	91.73	1.12	91.49	2.77	88.82	0.96	92.38	1.34	88.55	1.55	88.13
14	1.08	93.10	1.90	82.39	1.62	90.08	2.33	84.72	0.88	92.04	1.01	92.02	0.99	92.50	2.57	89.64	0.87	93.05	1.36	88.37	1.48	88.65
15	1.13	92.78	1.65	84.70	1.66	89.86	2.04	86.63	0.83	92.51	0.98	92.29	1.00	91.79	2.33	91.39	0.81	93.55	1.50	87.16	1.37	89.20
16	1.01	93.48	1.59	85.25	1.66	89.86	1.83	88.00	0.70	93.62	0.89	92.97	0.90	93.16	1.98	92.00	0.78	93.78	1.15	90.14	1.23	90.60

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17	1.08	93.08	1.39	87.11	1.21	91.13	1.78	88.34	0.69	93.68	0.88	93.09	0.90	93.03	1.61	93.50	0.63	95.00	1.02	91.22	1.16	91.16
18	1.04	93.33	1.37	87.31	1.39	91.44	1.73	88.55	0.67	93.92	0.82	93.23	0.86	93.76	1.51	93.16	0.64	94.91	0.97	91.68	1.13	91.35
19	0.97	93.81	1.22	88.55	1.63	90.02	1.51	90.09	0.56	94.88	0.83	93.45	0.75	94.31	1.47	94.09	0.63	94.95	0.98	91.63	1.02	92.24
20	0.87	94.45	1.07	89.91	1.42	91.34	1.58	89.64	0.62	94.33	0.83	93.37	0.73	94.42	1.20	95.16	0.59	95.26	0.93	92.07	0.95	92.71
21	0.93	94.08	1.11	89.72	1.46	91.03	1.44	90.55	0.60	94.54	0.76	93.65	0.80	93.98	1.12	95.49	0.54	95.64	1.07	90.88	0.94	92.78
22	0.90	94.23	1.13	89.56	1.33	91.83	1.31	91.41	0.58	94.73	0.73	94.19	0.71	94.58	1.08	95.62	0.59	95.34	0.86	92.18	0.92	92.94
23	0.95	93.92	1.10	89.79	1.23	92.44	1.29	91.56			0.78	93.81	0.79	93.98	1.20	95.13	0.61	95.10			0.87	93.38
24	0.78	95.02	1.02	90.50			1.47	90.39			0.69	94.53	0.72	94.52	1.07	95.66					0.84	93.63
25			0.93	91.39			1.27	91.68			0.66	94.74										
26							1.17	92.33														
After 4 months.			1.99	81.62	2.04	87.51			1.09	90.14			0.94	92.85	1.80	90.29	0.90	92.08				
Average rate....	3.16		3.85		3.90		4.55		2.61		2.76		2.59		5.75		2.46		2.86		3.74	
M. V.....	2.78		2.56		3.15		3.25		2.31		2.46		2.17		4.08		2.18		2.17		2.99	

The rate is the time in seconds required to deliver a single card.

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TABLE 2

Shows the relation between the learning-rate of mapping the location of the boxes and the rate of distributing the cards

NUMBER OF EXPERIMENT	PLANS OF DISTRIBUTION: SPADES THROWN TO DIAMONDS, AND DIAMONDS TO CLUBS, AND CLUBS AND HEARTS TO THEIR RESPECTIVE BOXES																																		CLUBS TO HEARTS, HEARTS TO SPADES, AND SPADES AND DIAMONDS TO RESPECTIVE BOXES									
	I				A				G				E				C				J				F				B				H				D				K			
	Date	R't	Wrong	Rate	Date	R't	Wrong	Rate	Date	R't	Wrong	Rate	Date	R't	Wrong	Rate	Date	R't	Wrong	Rate	Date	Rt.	Wrong	Rate	Date	R't	Wrong	Rate	Date	R't	Wrong	Rate	Date	R't	Wrong	Rate	Date	R't	Wrong	Rate	Date	R't	Wrong	Rate
				′ ″				′ ″				′ ″				′ ″				′ ″				′ ″				′ ″				′ ″				′ ″				′ ″			
1	Oct. 19	1	26	10 54	Oct. 14	1	21	13 35	Oct. 15	5	10	11 24	Oct. 15	3	9	9 35	Oct. 16	3	5	14 10	Oct. 19	1	7	10 10	Oct. 17	2	39	10 58	Oct. 16	0	8	9 23	Oct. 14	0	9	16 10	Oct. 16	0	39	12 27	Oct. 15	0	0	11 20
2	Oct. 26	8	23	7 29	Oct. 21	6	23	8 54	Oct. 22	6	12	6 51	Oct. 22	5	30	7 11	Oct. 23	5	6	11 10	Oct. 26	3	8	7 30	Oct. 24	5	34	8 42	Oct. 23	2	36	8 43	Oct. 21	0	10	14 03	Oct. 18	3	36	10 18	Oct. 22	4	9	10 07
3	Nov. 2	26	12	6 45	Oct. 28	11	27	8 11	Oct. 29	12	7	6 21	Oct. 29	8	8	7 11	Nov. 6	10	10	10 28	Nov. 2	7	8	5 50	Oct. 31	11	28	7 07	Oct. 30	0	39	7 51	Oct. 29	5	15	11 12	Oct. 23	4	35	9 40	Nov. 1	5	5	8 50
4	Nov. 9	34	5	3 56	Nov. 4	30	9	5 38	Nov. 5	20	10	4 13	Nov. 5	11	14	5 12	Nov. 13	18	6	6 50	Nov. 9	16	4	5 25	Nov. 07	17	22	4 40	Nov. 6	7	7	6 21	Nov. 4	7	12	9 21	Nov. 1	7	32	8 17	Nov. 5	7	4	7 09
5	Nov. 16	39	0	2 45	Nov. 11	39	0	4 02	Nov. 12	31	8	3 35	Nov. 12	21	9	3 46	Nov. 20	26	13	4 10	Nov. 16	22	9	3 49	Nov. 14	24	15	3 47	Nov. 13	11	8	5 19	Nov. 11	3	15	8 20	Nov. 6	14	25	6 32	Nov. 12	11	7	6 06
6	Nov. 23			2 07	Nov. 18			2 18	Nov. 19	39	0	2 07	Nov. 19	30	9	2 55	Nov. 27	32	7	2 51	Nov. 23	27	6	2 55	Nov. 21	32	7	2 30	Nov. 20	13	5	5 22	Nov. 18	5	9	7 55	Nov. 13	21	18	6 04	Nov. 19	16	20	4 21
7	Dec. 3			1 30	Nov. 25			1 50	Nov. 26			1 40	Nov. 26	35	4	2 18	Dec. 4	36	3	2 33	Nov. 30	31	3	1 54	Nov. 27	33	6	1 41	Nov. 27	22	3	4 21	Nov. 25	5	8	6 30	Nov. 20	30	9	4 10	Nov. 26	21	1	3 08
8	Dec. 7			1 26	Dec. 2			1 48	Dec. 3			1 28	Dec. 3	39	0	1 40	Dec. 9	39	0	2 20	Dec. 7	36	3	1 45	Dec. 5	34	5	1 22	Dec. 4	29	7	3 29	Dec. 2	7	10	5 45	Nov. 27	32	7	3 22	Dec. 3	27	2	3 04
9	Dec. 14			1 07	Dec. 9			1 31	Dec. 10			1 18	Dec. 10			1 14	Dec. 18			1 49	Dec. 14	39	0	1 24	Dec. 12	38	1	1 12	Dec. 11	34	1	3 11	Dec. 9	12	15	4 33	Dec. 4	39	0	2 54	Dec. 10	33	3	2 12
10	Dec. 21			0 58	Dec. 16			1 11	Dec. 17			1 10	Dec. 17			0 55	Jan. 8			1 47	Dec. 21			1 38	Dec. 19	39	0	1 03	Dec. 18	35	3	2 49	Dec. 16	17	6	3 56	Dec. 18			2 32	Dec. 17	35	0	1 57
11	Jan. 4			0 55	Jan. 6			1 12	Jan. 7			1 08	Jan. 7			0 51	Jan. 15			1 38	Jan. 4			1 15	Jan. 9			1 03	Jan. 8	38	1	2 17	Jan. 6	18	5	4 10	Dec. 19			2 09	Jan. 7	38	1	1 33
12	Jan. 11			0 52	Jan. 13			1 09	Jan. 14			1 02	Jan. 14			0 50	Feb. 12			1 32	Jan. 11			1 20	Jan. 16			0 54	Jan. 15	38	1	1 50	Jan. 13	18	7	2 37	Jan. 8			2 20	Jan. 14	39	0	1 16
13	Jan. 13			0 50	Jan. 20			1 02	Jan. 21			0 58	Jan. 21			0 44	Feb. 19			1 26	Jan. 13			1 10	Jan. 23			0 54	Jan. 22	39	0	1 43	Jan. 20	22	4	2 24	Jan. 15			2 04	Jan. 21			1 21
14	Feb. 15			0 45	Feb. 10			0 56	Feb. 11			0 51	Feb. 18			0 46	Feb. 26			1 24	Feb. 15			1 11	Feb. 13			0 53	Feb. 13			1 39	Feb. 10	25	1	2 14	Jan. 30			2 00	Feb. 11			1 17
15	Mar. 1			0 42	Feb. 17			0 59	Feb. 18			0 52	Feb. 25			0 43	Mar. 12			1 26	Mar. 1			1 18	Feb. 20			0 51	Feb. 19			1 26	Feb. 24	27	2	2 01	Feb. 6			1 46	Feb. 18			1 12
16	Mar. 8			0 40	Feb. 24			0 53	Feb. 25			0 47	Mar. 4			0 37	Mar. 19			1 26	Mar. 8			1 00	Feb. 27			0 46	Feb. 26			1 23	Mar. 3	30	4	1 43	Feb. 11			1 35	Feb. 25			1 04
17	Mar. 15			0 33	Mar. 3			0 56	Mar. 4			0 47	Mar. 11			0 36	Mar. 26			1 15	Mar. 15			0 53	Mar. 6			0 46	Mar. 5			1 12	Mar. 10	33	6	1 24	Feb. 19			1 33	Mar. 4			1 00
18	Mar. 22			0 33	Mar. 10			0 54	Mar. 11			0 43	Mar. 18			0 35	Apr. 2			1 12	Mar. 22			0 50	Mar. 13			0 44	Mar. 12			1 12	Mar. 17	37	2	1 18	Feb. 25			1 30	Mar. 11			0 59
19	Mar. 29			0 33	Mar. 17			0 50	Mar. 18			0 39	Mar. 25			0 29	Apr. 23			1 24	Mar. 29			0 51	Mar. 20			0 43	Mar. 19			1 04	Mar. 24	39	0	1 16	Mar. 4			1 19	Mar. 18			0 53
20	Apr. 5			0 31	Mar. 24			0 45	Mar. 25			0 38	Apr. 1			0 33	Apr. 30			1 14	Apr. 5			0 48	Mar 27			0 44	Mar. 26			0 57	Mar. 31			1 02	Mar. 12			1 22	Mar. 25			0 50
21	Apr. 26			0 29	Mar. 31			0 48	Apr. 1			0 41	Apr. 8			0 31	May 9			1 16	Apr. 26			0 35	Apr. 3			0 42	Apr. 2			0 58	Apr. 7			0 59	Mar. 19			1 15	Apr. 1			0 49
22	May 3			0 31	Apr. 7			0 47	Apr. 22			0 37	Apr. 22			0 30	May 10			1 09	May 10			0 48	Apr. 10			0 38	Apr. 9			0 59	Apr. 21			0 57	Mar. 26			1 08	Apr. 8			0 48
23	May 7			0 32	Apr. 21			0 49	Apr. 28			0 41					May 14			1 04					Apr. 24			0 41	Apr. 23			0 58	Apr. 28			1 03	Apr. 2			1 07	Apr. 22			0 45
24					Apr. 28			0 41	May 13			0 38													May 1			0 36	Apr. 30			0 54	May 12			0 56	May 14			1 16	Apr. 29			0 43
25																									May 8			0 35	May 14			0 49					May 27			1 06				
26																																				May 28			1 01				

R't = The number of boxes mapped correctly. (39 boxes were used.)
Wrong = The number of boxes mapped incorrectly.
Rate = The time required to deliver a pack of 52 cards.

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TABLE 8

Showing percentages of feeling qualities according to direction of movement and substitution and non-substitution of suit

	MOVEMENTS	↗				↘				↖				↙				↑				↓				→				←			
		+	−	o	t	+	−	o	t	+	−	o	t	+	−	o	t	+	−	o	t	+	−	o	t	+	−	o	t	+	−	o	t
Plan I	In a series	69	11	18	2	71	14	9	6	50	29	15	6	49	29	20	2	67	16	11	6	65	16	16	3	75	13	11	1	43	23	32	2
		71	5	18	6	70	10	15	5	52	29	14	5	61	21	16	2									89	4	6	1	60	19	13	8
		Av. 70	8	18	4	70.5	12	12	5.5	51	29	14.5	5.5	55	25	18	2									82	8.5	8.5	1	51.5	24	22.5	5
	As an individual movement	66	16	17	1	64	17	18	1	41	37	16	6	44	37	16	3	53	23	20	3	51	29	20	1	78	11	11	0	52	14	34	0
		72	11	15	2	71	12	16	1	49	32	18	1	59	23	17	1									79	7	14	0	58	28	14	0
		Av. 69	13.5	16	1.5	67.5	14.5	17	1	45	34.5	17	4	51.5	30	16.5	2									78.5	9	12.5		55	21	24	
Plan II	In a series	69	25	6	0	82	18	0	0	71	22	7	0	61	39	0	0	83	11	6	0	33	67	0	0	100	0	0	0	22	78	0	0
		94	6	0	0	89	11	0	0	45	55	0	0	66	33	1	0					89	11	0	0	93	7	0	0	94	6	0	0
		Av. 81.5	15.5	3	0	85.5	14.5	0	0	58	38.5	3.5	0	63.5	36	0.5	0					61	39	0	0	96.5	3.5	0	0	58	42	0	0
	As an individual movement	70	25	5	0	87	11	2	0	80	20	0	0	72	28	0	0	75	20	5	0	60	30	10	0	90	10	0	0	20	80	0	0
		94	6	0	0	90	5	5	0	45	52	3	0	43	57	0	0					90	7	3	0	100	0	0	0	95	5	0	0
		Av. 82	17	2.5	0	88.5	8	3.5	0	62.5	36	1.5	0	57.5	42.5	0	0					75	18.5	6.5	0	95	5	0	0	57.5	42.5	0	0

Note: The percentages above are calculated for any direction on the sum of the +, −, 0 and t totals for that direction as base, e.g., in the first division 69 per cent under + means that the sum of the + reports of all Right Upward movements is 69 per cent of the total of the sums of +, −, 0 and t reports for that movement. Movements accompanied by change of suits are in heavy type; unaccompanied by change of suits in light type.

+ = Pleasant; − = Unpleasant; 0 = Neutral; t = Tense.

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the distributing position made the movements at as uniform rate as possible (about one movement every two seconds) in the order and manner similar to a regular trial and reported its feeling quality as soon as executed. The subjects without exception conceived the movements as being made on a vertical plane, similar to that of the case, and as consecutive from box to box beginning with the Q of C and ending with the A of H. The results of the "memory report"[3] of the feeling tone of movements do not appear in table 3.

In the earlier periods of the practice, the left hand remained comparatively stationary, holding the pack, while the right hand performed in order the operations of "going," "taking," "fetching" and "tossing." But from the fourth to the eighth period "going" and "fetching" were eliminated by most of the subjects, and the left hand was trained to move in unison with the right and to serve it with a card at the proper receiving box. As expressed by one of the subjects, "The left hand became a baggage car for the right." Besides, the left thumb was trained to feed the cards one at a time to the thumb and forefinger of the right hand. This reduced the work of the right to "taking" and "tossing." Here again the individuality of the subjects became pronounced through the skill and rate with which the superfluous movements were eliminated.

3. The third group of subsidiary results, to appear in a separate paper, consists of answers made by the "receiving" and the "distributing" mail clerks to a questionary sent to 22 post offices in the larger cities of the United States. Some 320 copies of the questions were distributed and from these 180 apparently reliable answers were received.

DISCUSSION OF RESULTS

A. The acquisition of skill in card-case distribution

Since the learning process is here used as a method for determining the nature of the relations between the acquisition of

[3] The "memory report" of the feeling qualities of movements runs strikingly parallel with that of the "perceptual" or actual report.

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skill and feeling, it becomes doubly important to describe the factors involved and to indicate their interdependence as far as possible. In the main, the case distribution, viewed on the conscious plane, reveals six phases: (1) learning the plan of the work; (2) forming connections between a card and its box-label, i.e. "establishing card;" (3) learning the location of the boxes, a study in *place memory;* (4) learning the sequence of the stacked cards; (5) forming a synthesis between the order of the cards and the sequence of movements; and (6) the synthesis becomes automatic.

1. Learning the plan of the work. Plan 1 required that spades be thrown to diamonds, diamonds to clubs, clubs to clubs and finally hearts to hearts' boxes; in terms of boxes it meant diamond, club, club, heart, repeated 13 times in every distribution of the pack. As might be anticipated, the plan was learned by several forms of repetition, with speech-motor predominating. IDlP.[4] "Directions repeated by incipient vocalization every time a new card was held for delivery."

The first object of the repetition was merely to learn the plan, requiring with but few exceptions not more than three periods, and in some only two. The two cards, spades and diamonds, plan I and clubs and hearts in plan II, involving mental substitution, required the bulk of the repetition. The repetition continued two to four periods after the plan had been learned; this excess in the earlier periods occurred at hesitation points, of when confirmation was desired, or as a precaution against error and confusion, or to clarify the process, or to prevent mind-wandering, or to control movement; and finally it was resorted to as an inhibiting measure against the effects of the unpleasant stimulus.

7D3P. "Clubs to clubs and hearts to hearts is becoming automatic, but it is some effort to translate spades to diamonds and less effort to change diamonds to clubs." 40D11P. "I found myself repeating the plan, it increases certainty of movement. If I trust to movement

[4] 1P1D is an abbreviation for first period, first distribution; 2P2D an abbreviation for second period, second distribution, etc.

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alone I do not feel as secure as when I repeat the plan. It is like seeing a person and then hearing him talk, or like trying to do something in the dark and then doing it in the light." (An exaggerated figure, perhaps, but it indicates the function of speech on the control of attention and on overt movements) 6D3P. "I am more conscious this period of the plan than in the preceding. Repeated the plan as a fence against the noise of the tin cans."

It was quite noticeable that unpleasant stimulus increased repetition.

2. Forming connection between the cards and their box-labels. This task is closely related to the preceding and in fact the two processes are mutually coöperative. But associating a card with its assigned box, under the conditions, involved more than mere connection between like or unlike colors, and between like or unlike forms; because the association of the *unlike* was roughly in opposition to the old association of the *like* (initial inhibition), e.g., connecting S with D involved suppressing the normal bond of S with S, the inhibiting force of which depended to some extent on the subject's familiarity with cards. The typical reports given below show the rise and fate of the mental operations employed.

Normal conditions. Subject J. 3D2P. "With heart in hand I look at once for *empty* box bearing corresponding number; and visualise the box-label of the card whose suit has been mentally changed. Diamond labels are sometimes confused with heart labels." 5D3P. "I am held back by cards requiring translation for fear of making a wrong throw. When before the case a card in hand recalls the box-label in visual-motor imagery, when away from the case labels appear in visual imagery only."

Pleasant stimulus. Subject A. 17D6P. "Give no attention to spades, they are the same as diamonds. Do not look at labels anymore. Imagery of box label is visual-kinaesthetic. Caught myself placing cards without looking at them or the box." Subject C. 6D3P. "I could not get visual image of diamonds when I held spades in my hand. Passed diamond boxes unnoticed and when discovered, it annoyed me." 7D3P. "Substitution is growing easier in both auditory and visual imagery." 9D4P. "The distributing process is in

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three stages for me. (1) Perception of card and immediate movement, and these movements are correct, (2) perception of card, hesitation, image of movement (kinaesthetic) and then the movement, (3) perception of card, hesitation, no imagery, followed by searching for the box; these are either the more central boxes or boxes receiving translated cards." Subject D. Plan II. 7D4P. "Began without recalling the plan, so when I threw clubs to hearts felt a bit uncertain for a moment. Dispelled uncertainty by mentally repeating plan." 14D6P. "Have long since quit repeating plan, but when a card appears I passively repeat the box label. The imagery is auditory and kinaesthetic."

Unpleasant stimulus. Subject E. IPID. "Look at card to be delivered and call name of its box-label while searching for it." (This subject works rapidly, energetically, places his hands in the boxes as if measuring while searching for others.) 3D2P. "With card in hand I mentally change the suit, and then if any imagery of box, I follow the cue to the box." 9D4P. Knew I was not moving rapidly but felt that movements were efficient and believed that I could find any box on short notice. I notice that the box-label, 4 of clubs is losing its visual imagery but the movement to wards the club box is functioning, that is I deliver by the kinaesthetic imagery of the movement." Subject F. 2D2P. "I draw a card from the deck, name its place orally. Sometimes the perception of the card brings the name of the translated card and then the place, then again the card may bring the name and visual image of its box-label." 5D3P. "See the card and at once visual imagery of box-label no kinaesthetic imagery of movement." 12D5P. "Visual imagery for the labels is fading in some instances." 35D10P. "I still repeat spades to diamonds only as they are the cards that still trouble."

These introspections indicate that "establishing card" conforms in a schematic way to the so-called constrained association." Here the perception of the card is comparable to the perception of the "key-work," and the box-label with which it should connect is comparable to the class of associated objects determined by the key-wcrd. But in this instance there are two degrees of constraint. The first is very slight when card and box-label are alike. Constraint here consists in requiring a definite box rather than any box, e.g., heart to heart, club to club. The formation of the associations in the first case offered no difficul-

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ties save a few instances in which hearts were confused with diamonds and spades with clubs. The learning stages are: (1) The subject rehearses the plan applicable to the card in hand. (2) There is an incipient repetition of the plan reinforced by visual imagery of box-label in some cases, or by kinaesthetic imagery in others. (3) There is a gradual fading of all forms of imagery relating to box label and the distributing process becomes automatic.

In the second degree of constraint, when card and box-label are unlike, forming the bond is more difficult for two reasons: First, the unlikeness between card and box-label resists on its own account the formation of an association; and, second, the strong suggestive tendency to deliver the card to the box to which it normally belongs forms a decided interference; the attraction of like for like functioned as in initial inhibition to the growth of association between card and unlike box-label. It was also found in plan I that the inhibitions of spades to diamonds were greater than diamonds to clubs. This was explained on the ground that boxes labeled diamonds were visited one half as often as those labeled clubs. The subjects repeated that portion of the plan involving spades to diamonds from four to twelve periods after they had neglected hearts and clubs altogether. These facts are stressed here not only on account of their bearing on the correlation between movements and their accompanying feelings, to be considered below, but also for their bearing on the question of interference between associations involving more or less contrary responses to the same stimulus.[5]

3. Learning the location of the boxes. A study in place-memory. Does the feeling quality of a movement used in delivering a card facilitate or inhibit learning the position of the receiving box? A preliminary investigation made by Kline and Owens (25) had already shown that boxes on the perimeter of

[5] This question originally discussed by Münsterberg for motor responses and further developed by Bergström and Bair has received recent attention by Culler in his monograph on "Interference and Adaptability," Columbia University, 1912.

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the case are the first located; boxes in and about the center are the most difficult; while boxes bearing labels of face cards are likely to be promptly and easily located. The study also showed that the ability to recognize the location of the boxes exceeded the memory of their positions by fifty to seventy-five per cent, a fact already demonstrated by Kirkpatrick and also by Hollingsworth (18). The present investigation showed similar differences. Interest here, however, centers in the memory of positions. The facts of place-memory were obtained by having the subjects map the boxes on blank forms containing 54 compartments representing those of the case. These mapping exercises and the observation of the case during the distribution furnished the means for learning the position of the boxes. The mapping was done from memory either between the distributions or after the final distribution of a period.

The reports of the subjects are not conclusive that either P or U stimulus favored or hindered mapping the boxes. A study of tables 1 and 2 shows that the time for mapping the boxes varies with subjects taking P stimulus from 5 to 13 practice periods, and with those taking U stimulus varies from 6 to 19 periods; while normal conditions required from 5 to 12 periods, thus giving slight evidence that U stimulus may have checked the mapping. The rate of mapping is affected by quite other factors. Seven of the subjects made a deliberate effort and used various devices to locate the boxes, while four subjects made no special effort, but relied on trial and error and on kinaesthetic cues. The active study not only corresponds to an early location of the boxes, but is directly associated with the initial rise shown by the learning curves. These seven subjects mapped the entire 39 boxes in an average of seven periods, and distributed the entire deck at an average speed of 51 seconds in the nineteenth period. Those who made no active effort mapped the boxes in an average time of thirteen periods, and attained an average speed of 66.75 seconds to deliver the deck at the nineteenth period.

This apparent correlation between the growth of memory in locating the boxes and rate of speed does not prove, however,

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that the ability to chart the boxes is a function of speed. The short life of the imagery for the position of the boxes, before the achievement of the higher rates of speed, indicate that fact. Rapidity in memorizing the positions seems to depend on differences in attitudes and in modes of attacking the problem. The ready recognition and use of minor aids throughout the learning was distinctly an individual trait and showed great variation among the subjects.

Conscious efforts were directed: first, to learning the four corner boxes; second, to filling in the four sides or edge boxes; third, to locating the inner boxes with reference to the edge boxes (and here those adjacent had an advantage, while those further in were located by the number of boxes removed from the side and from the end); fourth, to grouping the boxes of the same number bearing different suits; fifth, to fixing the box by the value of its cards in certain games; sixth, to locating contiguous boxes correlated with sequential movements (pairing the boxes); seventh, to locating boxes that were the termini of long sweeping movements.

Since the maps were made at every period until final completion, it was possible to arrange the results in a time series and thereby show the growth of place memory. In table 2 the series is thus arranged under the caption Rt. (right location), and an inspection of this column shows that, when the number reaches 39—the number of boxes used—the rate of distribution, with a few exceptions, rapidly increases; the column headed "rate" in table 2 shows this.

Since place-memory is a temporary function of skillful movements, it is necessary to see if there is any causal relation between the nature of a movement, and the growth of such memory for the point toward which the movement is made. For this purpose the time order in which each box was first located by *all* the subjects was determined, and then the probable causes for such priority examined.

The time order in which a box was located by all subjects was done by counting the number of correct locations given a card by all subjects taken collectively at the same period.

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No card could be located more than nine times in plan I during any given period, since there were only nine subjects. It will be recalled that subject H required 19 trials to make a perfect map. If all subjects had required that number of periods it is possible that some of the cards would have been located nine times nineteen, or 171 times. Table 4 shows in the last column but one the number of times each card would have been located had all the subjects continued charting from the time of its location until H completed his in the nineteenth period. The last right hand column shows the percentage of this number (for each card) to the possible number of locations. For example, the 4 of C under the assumed conditions would have been located 164 times, that is 95.9 per cent of 171, the highest possible number of times. The cards were then arranged in a descending series, beginning with the 4 of C and ending with the 5 of H which had been located 105 times, or 61 per cent of the possible number. This serial arrangement shows that eighteen peripheral cards, with the exception of the 9 of C and the Q of H, two inside cards, were given the highest number of locations.

The peripheral cards of the case were located in the following order: upper left, upper right, and lower right. The lower left, 8 of D (plan I), ranks ninth. The subjects regarded lower left hand boxes difficult to remember: (1) The boxes seldom came within range of the visual field, (2) This particular box 8 of D (diagram 1) was reached by an awkward movement—down vertical with the right arm passing across and in front of the body, (3) The 8 of D box received a card requiring substitution, 8 of S (diagram 2; plan 1 first 13). Edge boxes rank next to those on the corner of the case: their nearness to the corner boxes, and their conspicuous position— they have roughly 25 per cent less distraction than the inner boxes—seem to account for their priority over the central boxes. These reasons appear to suffice when outer boxes are compared with inner, but they do not explain priority of one edge-box over another. A bit of objective evidence mentioned above is repeated here to introduce principles to be more rigidly applied and tested in connection with the problems of inhibition, error and feeling tone. (1)

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TABLE 4

Plan I showing actual and comparative number accurate locations of boxes from maps

			I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV	XV	XVI	XVII	XVIII	XIX	PER CENT
			9	18	27	36	45	54	63	72	81	90	99									
1	P 4	Clubs	5	12	20	**29**															164	95.9
2	P 6	Clubs	4	9	16	24	32	41	49	**58**											157	91.8
3	P 6	Hearts	0	4	11	20	**29**														155	90.6
4	P 10	Clubs	1	5	10	16	23	31	**40**												148	86.5
5	P K	Hearts	1	2	8	16	24	32	40	48	56	**65**									146	85.4
6	P K	Clubs	2	5	9	15	22	29	37	45	**54**										144	84.2
7	P Q	Clubs	0	5	8	14	21	29	37	45	**54**										144	84.2
8	P J	Hearts	1	2	6	13	21	29	38	45	53	**62**									143	83.5
9	P 8	Diamonds	0	0	3	10	18	26	34	42	**51**										141	82.4
10	P 8	Clubs	0	1	4	10	16	24	32	**41**											140	81.8
11	P 3	Diamonds	0	1	4	10	17	25	33	41	**50**										140	81.8
12	P 7	Diamonds	0	1	2	8	16	24	32	40	48	**57**									138	80.7
13	P Q	Diamonds	0	0	3	8	14	21	29	**38**											137	80.0
14	P J	Clubs	0	1	6	12	19	26	33	40	47	**56**									136	79.5
15	C 9	Clubs	0	1	5	10	16	23	31	39	46	55	63	71	79	**88**					133	77.7
16	P 7	Clubs	0	0	1	6	12	19	26	34	**43**										133	77.7
17	C Q	Hearts	0	2	5	7	13	20	28	36	44	52	60	**69**							132	77.0
18	P 10	Hearts	1	3	5	7	11	18	25	33	42	**51**									132	77.0
19	P 9	Diamonds	0	0	1	6	13	20	27	34	42	50	58	**67**							130	76.0
20	P 4	Diamonds	0	0	1	4	11	18	26	34	42	50	58	**67**							130	76.0
21	C A	Diamonds	0	0	1	5	11	17	25	33	41	49	57	65	73	81	**90**				126	73.7
22	C 2	Clubs	0	1	3	7	12	19	26	34	42	50	58	66	74	82	90	98	**107**		125	73.1
23	C J	Hearts	0	0	1	3	9	15	22	29	36	44	**53**								125	72.4

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24	C A	Clubs	0	0	1	3	8	14	21	28	35	43	**52**							124	72.4
25	C A	Hearts	0	0	3	5	10	16	23	31	39	47	55	63	71	79	87	**96**		123	71.9
26	C 2	Hearts	0	0	0	2	5	12	19	27	35	43	51	**60**						123	71.9
27	C 10	Diamonds	0	1	2	6	12	17	23	30	37	45	53	61	69	77	85	93	**102**	120	70.2
28	C 3	Clubs	0	0	0	1	5	12	19	27	35	43	52	60	68	76	84	**93**		120	70.2
29	C 6	Diamonds	0	0	1	2	4	11	18	26	34	42	50	58	66	74	**83**			119	69.6
30	C 5	Clubs	0	0	0	1	5	11	17	24	32	40	48	56	**65**					119	69.6
31	C 4	Hearts	0	0	0	1	4	10	17	24	32	40	48	56	**65**					119	69.6
32	C K	Diamonds	0	0	1	3	7	12	19	27	35	43	51	59	67	75	83	91	99	**108**	117	68.4
33	C 2	Diamonds	0	0	1	3	6	9	15	23	31	39	47	55	64	72	80	**89**			116	67.8
34	C 9	Hearts	0	0	1	3	7	13	19	26	33	40	47	54	62	70	78	86	94	**103**	112	65.5
35	C 7	Hearts	0	0	0	1	3	7	14	22	30	38	46	54	62	70	78	86	94	**103**	112	65.5
36	C 8	Hearts	0	0	0	2	4	8	14	21	29	37	44	52	60	68	76	84	92	**101**	110	64.3
37	C 5	Diamonds	0	0	0	0	3	7	13	19	27	35	42	50	58	66	74	82	90	98	**107**	62.5
38	C 3	Hearts	0	0	0	1	4	8	12	18	26	34	41	49	57	65	73	81	89	97	**106**	62.0
39	C 5	Hearts	1	0	0	1	3	6	10	16	23	30	38	46	54	62	70	78	**87**		105	61.4

Roman numerals indicate number of period.
P = Boxes located on periphery.
C = Central boxes.
Arabic numerals show actual number of cumulative mapping of any card for any period.

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Edge boxes that are adjacent to corner ones favor place-memory e.g., 3 of D, being adjacent to 6 of C was learned prior to Q of D (see diagram 1). (2) Other things being equal boxes at the termini of long sweeping movements take priority in place memory, e.g.,8 of C, being the terminus of two comparatively long movements, is considerably prior to J of C (diagrams 2, a and b; second, third and fourth 13). (3) "Resident" boxes favor place memory, e.g., J of H was learned prior to 8 of D and 10 of H prior to 9 of D, that is, boxes receiving cards unlike their labels. This principle did not hold with the club boxes, despite the fact that each box was visited twice in every trial, and, on the principle of repetition, should have taken priority over the heart boxes. The evidence shows, however, that diamonds to clubs either operated as a retroactive inhibition, and thus annulled the principle of repetition, or that the normal association of D to D not only interfered with D to C, but also disturbed the normal association of C to C. (4) Boxes receiving cards delivered with a rightward movement, other things being equal, were located prior to boxes receiving cards delivered by movements toward the left, e.g., the 9 of D was located prior to 4 of D (diagram 2, plan I, second and fourth 13, and table 4).

The principles governing the location of the lateral boxes operate similarly on the inner boxes. The *third* principle, that of "resident" boxes finds illustration in the 2 of H being located prior to 6 of D (diagram 2, plan 1, first 13) and by the 4 of H prior to the 2 of D. The former pair are approached by movements to the right, they differ only in that the 2 H is "resident," and the latter pair are approached by movements to the left, but the "resident" box is prior in being located. The *fourth* principle, *rightward movement,* is well illustrated by the 2 of H being located much earlier than 4 of H; both are "resident" and adjacent to edge boxes. They differ in *one* element only, the 2 of H is approached by a movement to the right and 4 of H by a movement to the left. By the same principle the 7, 8 and 9 of H, three boxes, were located prior to 3 and 5 of H. Attention is called to the fact that the 5 of H is a "resident" card and adjacent to a lateral box, the J of C, and yet despite these

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advantages the short cramped-up-leftward movement placed it at the foot of the list (table 4).

The validity of the fourth principle may be questioned on the ground that many cards were located before the *direction* of the movements had become sufficiently definite to serve as a cue for place-memory. A study of the synthesis of the movements, given later, shows that the "kinaesthetic feel" for the direction of certain boxes was present even in the second trial of the first period. Besides the investigations of Müller and Schumann (29) Bergström (3) Smith (36) Culler (11) and others have placed beyond dispute the immediate and persistent effects of kinaesthesis, particularly of hand and eye in associational process.

This lengthy consideration of the growth and of the factors involved in place-memory shows, at least, that two types of movements, (1) those of a relatively long sweep, (2) those of a rightward direction, facilitate the growth of such memory and hence, indirectly, aid in acquiring skill for card distribution.

There is evidence of a *fifth* principle which not only inhibited the growth of place-memory but disturbed the memory for the boxes throughout the entire practice. The principle may be stated as the *cumulative effect of two successive card substitutions.* This was brought about by the conditions of the experiment. The order of the suits, C, D, S, H, and the plan of substitution became in terms of box-labels C, C, D, H, i.e., D went to C and S to D. This required two substitutions in immediate succession, the climax coming with "Spade to Diamonds" which not only accounts, apparently, for the late location of the K of D, 2 of D and 5 of D, but also for the fact that six cards of the heart suit are among the last nine (31 to 39, table 4) cards to be located. The cumulative effect persisted even to delivering H to H (following two substitutions) which otherwise formed the easiest part of the work.

4. Learning the sequence of cards "stacked order." Learning the stacked order of the cards, like the location of the boxes, while probably not essential to the maximum rate ultimately attained by any subject, materially contributed to the initial speed and to

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the consequent form of the learning curve. Besides the manner of learning the stacked order gives added force to the rôle of kinaesthesis as it affects the different phases and stages of learning.

The methods used in learning the order, as shown by the reports of the subjects, were three: rational, sectional, and trial and success.

The rational method consists in the application of sense experience to the solution of a novel problem. The order of the suit was learned largely by visual impressions supported and checked by kinaesthetic. The subject reasoned that the 4 suits were repeated 13 times and the numerical order 4 times, then by matching number and suit for the first 13 a pattern was formed by which the entire series was mechanically completed.

The sectional method consisted in applying the suit order, clubs, diamonds, spades, hearts to the numerical order as the latter was learned. This caused the subject to learn isolated sections here and there in the *run* of the 52 cards. Subject E at the sixth period tried to write the order in terms of movements and boxes. It appeared thus Q of C, 5 of C, 6 of D; 4 of H, 3 of C, 10 of C; 8 of D, 2 of H, K of C; 7 of C, 9 of D, J of H; A of C, in groups or sections of three each. Spades had been completely suppressed! This method continued until the ninth period when he wrote out the first 13 correctly and began on the second with the Q of C. He suddenly realized that Q of C was the *first* card delivered, and that the fourteenth card must be of some other suit. He remembered that it was Q of D and this suggested the Q of S as another possible queen. It then occurred that he had been omitting spades altogether from his records. He tried again, arranging the cards in groups of *four* and wrote out 46 of the series correctly, omitting four from the third 13. Evidently he had not seen the relation between the two orders: suit and numbers. At the tenth period the entire series was written correctly.

The numerical order was determined largely from the sequence of movements. The spade is an absent element. The order of the suit was new until the 9P, when I saw it would apply to groups of four; it had not occurred that the order of the suit might be applied to the numerical order for discovering the entire series.

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The trial and success method was slow and tedious indeed. In one case the series passed in review 897 times without the subject detecting any plan in the suit or in the numerical order. The kinaesthesis of the movements were largely relied upon and were sometimes rehearsed, mimetic fashion, in writing the series.

Summary for stacked order. *a*. Three methods were employed which appear to turn on the recognition and use of the order of the suit. If the suit order was recognized while the distributing process was well within the conscious stages, the subjects were likely to solve the order by the rational method. This was done by subjects A, G, I of plan I and D of plan II. If the subject depended on the sequence of movement and more or less on the visual imagery of the boxes, for writing the order, it is clear that the order would grow only with the increase of knowledge of sequence of movements. Although such knowledge was formed in the initial periods, it progressed comparatively slowly, and soon passed to the automatic or functional form. So that if the order corresponding to the movements was not written while the latter was on the conscious plane, it had to be solved by the trial and error method. This explanation applies particularly to F and largely to B, C and J. These latter did not see the relation between suit and order until the entire series was nearly completed by the trial and error method. In the case of C and B the rational method was suggested at the last moment by the results of trial and error. K, who used no other principle but trial and error, never succeeded in writing out the order. H learned the order of the suit at the eighteenth period, but never succeeded in writing out the entire series. Again if the subject forms habits, associations, synthesis of movements with comparative rapidity, then the sectional method is likely to be employed. This was E's type of learning. He often remarked, and his work showed, that he formed habits rapidly. His complete suppression of Spades and attempt to learn the order in terms of boxes is a case in point.

b. There exists a direct correlation between the manner and rapidity of learning the stacked order and the comparatively short interval of attaining high rates of speed. Subjects using

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the rational and sectional method make rapid initial progress, while those using the trial and error did not reach their maximum rates within the limits of the practice periods.

c. No correlation whatever is observed to exist between the conditions of practice, whether normal or with pleasant or unpleasant stimulus, and the rate of learning the order.

d. All subjects made use of the knowledge of sequence of movements. C, F, and G made considerable use of visual imagery of boxes in connection with sequence of movements.

e. Evidently learning the plan of the work, "establishing card," and locating the boxes, all alike tended toward the suppression of spades, and therefore interfered with the stack order arrangement. But mapping the box sustains a probable correlative relation to writing the order; for, since the order of the suit was written largely from the kinaesthetic imagery, and especially by those using the method of trial and error, and since it was found that the location of certain boxes was difficult on account of the nature of the movement approaching the box, it raises the expectation that these movements should affect the learning of the order of the cards involved. An examination of the records of C, F, J, E and even G show that the 2 of D, K of D, 5 of C, 3 of H and 5 of H were uniformly among the last cards to be properly arranged in the series, and it will be recalled that they were among the last cards to be properly located. This comparison finds support in later considerations.

f. While the order was being learned, and in some cases immediately afterwards, it was either regarded as of no service or as a hindrance. The next section indicates, however, that such knowledge rendered a positive service to all who learned by "rational" and "sectional" methods.

5. Synethesis between the order of cards and the sequence of movements. The previous section foreshadowed a synthesis between these two series of associations. The present section traces the progress of the synthesis through the several conscious phases to the automatic stages and also indicates the rôle of attention that accompanies the synthesizing process.

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The number of repetitions necessary to form a connection between any two consecutive movements, could not, under the conditions of the experiment, be determined with precision. "Find the box" was the dominant problem in the beginning, but with the growth of place-memory, it became possible to make consecutive movements in due order without false movements intervening. Some such experience must have occurred as a basis for establishing the association. The introspective reports show that several of the fifty-two mevements began to form associative sequences, on the average for the eleven subjects, at the eighth or ninth trial (M = 8″.3 with M. V. = 1.6.). The imagery for the boxes and the movements passes from a vague to a clear definite form and then fades out, as a rule the imagery for the boxes being the first to go. The duration of the imagery was exceedingly variable. "It appeared only to vanish" is a favorite characterization.

Subject I at the 4P says: "A few boxes are losing their individuality, some have never been known, and others have clear imagery but it is difficult to hold." In the next period, 5P, he says "Imagery of some of the boxes is fading out and delivery is becoming mechanical. In many instances the movements are well known, but the card going with the movement is vague." On the other hand there was no evidence to show that every movement and its terminal box passed through a definite stage of imagery. After eight to sixteen trials the subjects without exception expressed surprise at the execution of correct movements without conscious supervision. Subject A at the twelfth trial finds that "boxes own their places and that it would be annoying to toss just any card into the box." Subject B at the fourteenth period says: "There are no more long pauses, there is a general familiarity with the movement. My hand remembers what my head forgets. After I deliver one card it seems a guide to the next." G says 8P: "I know some places are automatic, for I start toward them and become conscious as I approach." 10D, "I found myself going to the right box oftener than I had anticipated I could." 12D. "I notice a *bodily tilt* toward certain boxes." J feels at the 22D, "the whole body lurch in a definite direction and it proves to be right." E at the 8D says: "I believe that the directions are becoming automatic, for

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the reason that chance movements are right as a rule." Subject I reports at the 9D "In many instances one delivery excites the next in order."

This initial stage may be regarded as one of vague motor attitude toward the movements. But the attitude does not endure long, for it is broken up at different points by the association of two, three and four movements.

At the twentieth trial J describes movements that are "smooth and flowing," and at the 28D says that "certain parts felt like going over an old trail." At 60D, "Attention is at times a little ahead of the card, I feel the need of better control of attention. At times I try to suppress it and say to myself, "Do not be so anxious, let cards go as they will." At the 70D, "I can anticipate the next box. This is done in terms of box and imagery of movement, with visual correction of hand. The constant use of the eye is merely for accuracy." 80D. "Over attention, or attention to what is automatic checks speed."

Subject I. 35D9P. "I feel that I can throw with some abandon, am centering attention on the order of the movements. There are places where the movements are obscure and hesitating and then there are other e.g. at the beginning, near the midle and at the end where I 'let her rip.'

40D10P. "The order of the cards, order of movement and the position of the boxes are all well known. The latter, position of the boxes, has grown clear again, since it is needed for accuracy. The movements would be too general if exact position of the boxes were not clearly known. The clearness and exactness are held by means of visual and motor imagery and geometrical relationships. Attention is on the receiving box. I do not look at the cards as I know the order and the movements. Each box in turn is a terminal box and attention is on the box and the card as it goes into the box. I do not read ahead in music, nor in print and likewise do not attend ahead in distributing cards. The whole fifty-two cards is becoming a unit. My memory for the pack is in four groups. I pause at the end of each group." 45D11P. "The correlation between order of cards and sequence of boxes is not yet complete. Memory for card, for box and for movement did not mutually support each other."

Subject F. 34D9P. "Tendency to throw cards in groups, first group includes the first 13. A group is forming at the end and growing

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toward the center." 39D10P. "Attention keeps ahead of the distribution if I know order of card and place of box, but if I know neither card nor box, I lapse back to perception of card and box."

44D11P. "The span of my attention is of three degrees (1) to card and then box, (2) card and box together, (3) card and group of cards and of movements." 49D12P. "I throw many cards without looking at them. This is done habitually. Here the attention keeps ahead of the performance just enough to pave the way for the next move."

These latter reports introduce a second stage of integration characterized by several marked features: (1) The synthetic groups were rapidly formed, sprang up in different parts of the series, 'mushroom' fashion as it were, and grew in extent and definition. (2) The control of the group involved attention to at least three objects, the clearness and stability of which were continually changing. At first attention was given to the card suggesting the group, to the boxes for the sake of accuracy and to the movements as a group. Attention to card was the first to cease, followed later by attention to the boxes within the group for some subjects, leaving the peculiar configuration of the synthesized movements as the last object of attention. (3) Two of the subjects, E and I found the size of the growing groups becoming unwieldy, so they deliberately sectioned them "in order to give the attention a purchase on its object." They coördinated the rhythm of the movements with the pulse and span of the attention. Both testify that, knowing the stacked order of the cards aided in this effort. (4) As the conscious phases of the groups dropped out, they were controlled more and more as units like individual movements, and the higher rates of speed at this point hinged on the ability to anticipate the next group while distributing the preceding. All subjects reached this stage which I have termed the *conscious group stage*, but a number of them were unable to control the groups to advantage. (5) The value of attention as a factor in a skilful performance depends both on its amount and *tempo*. Coover and Angell (10) found the amount and uniformity of attention coördinated with speed in typewriting. The inability to coördinate and grade the attention to the needs of the movements

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resulted in confusion, errors, in lapses and in consequent loss of speed. A perfect synthesis of movements, order of cards and order of boxes on the conscious plane means their functional coördination. One subject says 40 D10P. "These three factors now work together, they are at ready command, formerly they were more conscious and functioned separately." All parts of such a synthesis remained at the conscious level a comparatively short time. I have already given the order in which the factors fade out.

6. Order of cards and sequence of movements in the automatic stage. The ability to handle the synthesized groups of movements as units paved the way for a higher and final synthesis, which may be termed the automatic stage, the completion of which came slowly. It required more practice than the preceding and was accompanied by frequent relapses.

Subjects using the sectional and rational methods of learning the stacked order of cards and who recognize a change in the system of movements at the end of each 13, developed, while in the "conscious" group stage, out of the 52 movements, four distinct systems. The habit for the gross movements grew so stable and sure that the attention was not needed for anticipating them; once started they ran *solid* without break from Q of C to A of H. Two of the subjects mastered them so thoroughly that with closed eyes they executed the movements in mimetic fashion by tapping with index finger on the table or wall points similar in relative position to the boxes visited.[6] They were therefore free to use attention in the interest of accuracy and for the more delicate aspects of the delivery, such as position before the case, manner of holding the cards, use of the left thumb, manner of grasping the cards with the right hand. When the stacked order was learned by the sectional method only, based on movements and their terminal boxes, the entire 52 movements were sectioned into small groups of three and four each. For a time these groups were handled as units and anticipated with marked precision. The break and emphasis between the groups

[6] This voluntary rehearsal of the movements was a favorite exercise of E while the case was being prepared for the next distribution.

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gradually faded out and finally shot together into larger ones embracing twelve to fourteen movements. The limits of these larger groups were fixed for a time by the return of the queens, by the consequent radical change in the system of movements in the first half of the cards and by the return of the 5 of H and 5 of C in the last half being due to the difficulty of these movements. Consult diagram 2, plan I, third and fourth. 13's, to see the difficulties involved. The breaks at the Q of D (Q of S) and 5 of H were the largest and were finally bridged by the movement to the Q of D fading out, followed later by the 5 of H movement in a similar fate. By the seventy-fifth distribution one subject claimed to "sense" the whole process as one, the beginning and the end were well nigh in the same concept.

Only two subjects of the eleven made any considerable use of visual imagery in anticipating grouped-movements in this stage. While their descriptions do not show that they regarded it of doubtful value, their errors, hold-ups and speed records point in that direction. Attention is directed here to the functional value of imagery in learning, because it is called in question. Dr. Fernald (16) in a general discussion of the problem inclines to the affirmative view. My own results indicate that its value depends on the individual, so that the problem is one to be settled by individual rather than by general psychology.

Subjects who attained the higher records and worked easily in the automatic stage revealed two things: (1) That the final stage was made not merely because they were able to control attention and direct it to the proper objects involved in the distribution, but because they had also attended at the proper time to a number of accessories that furnished a substantial basis and framework out of which and on which the higher achievements were developed. (2) That rapid delivery developed difficulties, revealed hard and easy places and aroused feeling qualities not experienced in the lower stages.

It is in these two stages, the *conscious group* and the *automatic*, and particularly the former, that reliable evidence on the relation between feeling tones and the acquisition of skill is to be secured. These results are further considered in connection with the learning curves.

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B. The learning curves

1. Attitudes, hindrances, devices in learning. It was understood from the outset that the purposes of the experiment required the subject's best effort at all times; but no suggestion was made that competition should be practised. Fairly definite attitudes were taken, however, toward the matter from the beginning. Some regarded it as "good fun," a game of friendly contest and maintained that position throughout. A few appeared indifferent at first and even mildly disturbed when questioned as to the feeling tone accompanying poor or good records. This attitude was observed with some care and seemed to mutate as follows:

a. Indifferent, may even mildly resent imputation of trying to make a record.

b. Wonders how the present record compares with a previous one.

c. Wishes to know progress from trial to trial. (Did not show records lest it might affect speed.)

d. Inquires about the records of other subjects!

e. Feels elated or disappointed according to the character of the record.

Hindrances aside from those encountered in the distribution consisted in faulty adjustment of the body before the case, the failure to stand squarely on both feet, a clumsy use of hands and fingers and moving the body from the hips as a pivot.

Some of the more useful devices consisted in moving the body from the soles of the feet as pivots, rising on tip-toe with increase of speed, an advantageous use of the left thumb in feeding the cards to the right hand, keeping the eye just ahead of the distribution.

2. Characteristics of the learning curves. The curves are constructed from the results given in Table I. The number of the practice period is shown on the horizontal line and the time required in seconds and fractions thereof to deliver a *single* card is recorded on the vertical.

The graphs show three distinct sections for the most part: (1) A section of *maximum gains*, comparable to the usual initial

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rise, (2) a *middle section* (see arrow in Graphs III and V) and (3) a section of *diminishing gain.* The first section extends from 1P to 7P and involves from twenty to twenty-five distributions. During these seven periods the grosser features of the work were learned and partially habituated, involving plan of the work, connections between cards and box-labels, and recognition of box position. The larger inhibitions grew noticeably less, surprise being often expressed at the increased number of habituated movements. The percentage of improvement of the normal subjects during these seven periods was 79.5 per cent, of the first record; of the subjects using U stimulus it was 78.9 per cent, and for subjects using P stimulus it was 72.66 per cent, i.e., in one-third of the practice the original record had been reduced by about three-fourths of its length.

The middle section for both the normal and the combined curves extends from 7P to 11 and 12P or from 40 to 50 distributions. The immediate cause for the sudden bend in the curve at 7P is due to relatively small percentage of improvement at 8P. The indirect causes are several.

First. The unstable character of the newly formed habits was easily overcome by the wear and tear, incident to the distribution. They were disturbed by a conscious endeavor to learn the stacked order and to complete the place memory of the boxes. These two tasks, although begun in the maximum section had to wait until the simpler and grosser ones were learned. But now that the subjects were free to deliberately attack them, the attempt often proved disastrous to the earlier habits.

Second. Comparatively higher speed at this time revealed the necessity for improving the technique for handling the cards, for controlling the body and for eliminating wrong practice. This effort proved an additional embarrassment and persisted for several periods.

Third. At this time the movements were beginning to form in small groups at different parts of the 52 series, with wide breaches between them composed of single and isolated movements. This made the delivery uneven and fitful; the grouped-movements being executed with rapidity and accuracy, only to

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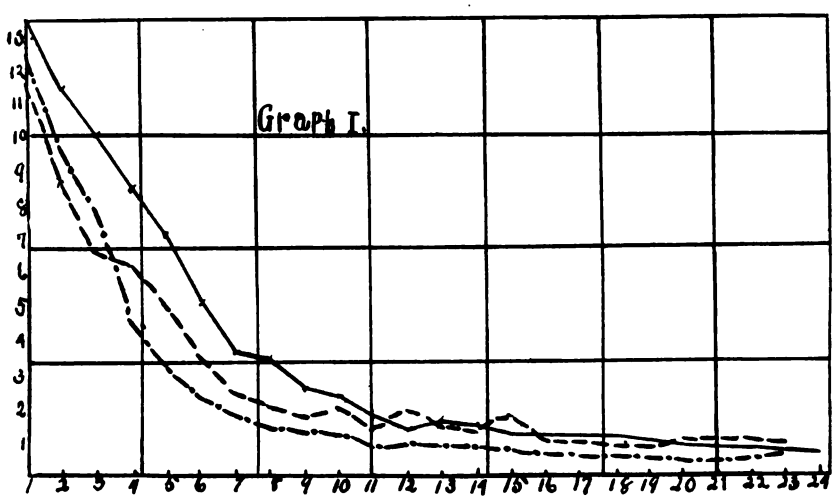
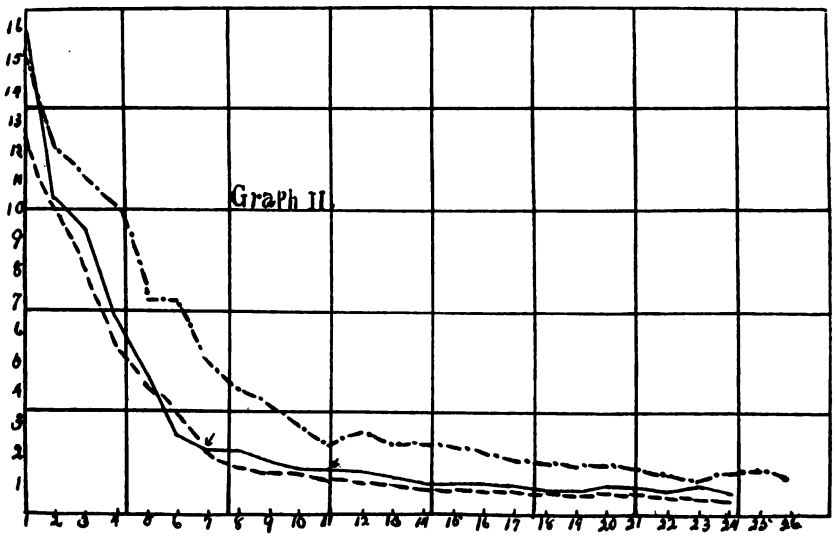


GRAPH I. NORMAL LEARNING CURVES

Subject K = ————————
Subject I = — - — - — -
Subject J = — — — — —

Ordinates indicate time in seconds to place one card. Abscissas indicate the number of periods of practice.



GRAPH II. STIMULUS CURVES

Plan I { Pleasant Stimulus: Subject A = ————————
Unpleasant Stimulus: Subject F = — — — — —
Plan II Pleasant Stimulus: Subject D = — - — - — -

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be followed by hesitation and false movements. One subject termed it the "bumpy and rocky" condition. These three hindrances operated with greater or less intensity throughout the middle section, depending largely on the subject and his methods of work. The first hindrance was readily overcome by I, while it continued beyond the limits of the section for J and K. The second class of hindrances was of minor importance to I and K, but proved serious to J.

Of course the general concave form of the curve calls for no comment as the percentages of improvement were per force lessened with the gradual achievements of higher records. The descriptions here given concern characteristics superimposed upon the general form.

The second joint or limit of the middle section occurs for the normal subjects at 9, 11 and 12P respectively, and in the combined curves (graph 5) for all conditions at the 11P, and marks the beginning of the section of diminishing gains. It should be observed that from this stage on to the end of the practice the margin left for further possible gain is comparatively small. Concerning the normal subjects, I had gained at the 10P 95 per cent, J at the 12P 93 per cent and K at 13P 94 per cent of his total percentage of improvement. Evidently over nine-tenths of their speed capacity is developed when one-half of the practice is completed: it required the remaining *one-half* to squeeze out the last tenth. But there is still the question, why did the second break appear at all and at these few adjacent periods. Considering the normal subjects only, the immediate cause is found in the relatively small gains in the tenth, twelfth or thirteenth period according to the subject. The evidence for the indirect causes is largely introspective. I's curve in graph 1 shows a very slight break at 9P. This suggested an examination of the record at 10P. It reads:

"I place emphasis on the aces as a means of orientation in memory, but in actual performance it is just one card after the other with the attention just a little ahead at each movement. The order of the cards, the order of the movements and position of the boxes are now working together and are at ready command," but in 11P this interest-

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ing introspection occurs "The work makes a heavy draft on the nerves! on the coördinating centers. The memory does not always readily supply the movement. The whole performance may become blurred for the lack of movement." J's curve beginning from 9P and extending to the end of the practice shows unusual fluctuations. This fact, the characteristic of the individual's habits of work on this problem, tends to obscure the second break. He sustains an actual loss in percentage of improvement at 10P and at 12P of 2.36 per cent and 1.81 per cent respectively. To select either 9P or 11 P as the breaking point appears arbitrary, so it seems best to take the entire region from 9 to 12 and to give the accompanying introspections. 10P. "There was no feeling of relaxation today. I was tense throughout. The body swayed to and fro and from side to side. The prophetic feeling for the movements was completely absent. I broke down completely during the 5P which was probably due to accumulation of effects (a) I had resolved all the week to make a good record before the holidays. (b) The preholiday stir-up, perhaps. (c) I heard other subjects talk about the higher records. (d) Errors of the preceding trials had unnerved me." 11P. "If I had a new case to learn I would go at it systematically, for I now find the need of knowledge as well as practice to make any headway." 12P "Dreaded to begin. I have reached a plateau and to go on I must bring both the position of the boxes and the order of the cards to consciousness. I seem unable to eliminate superfluous movements of hand and body." Concerning K, the second break occurs unmistakably at 12P, which suggests an examination of the next record. 13P. "The movements were not allowed full freedom. Held myself in check for fear I should lose control of movements. If I try to hurry I go wrong. There is over muscle tension and too much nervous energy used up in the distribution." My own notes record "Cards thrown with excessive force, hear them strike the box. Subjects shuffle feet. Rebuke themselves half audibly for mistakes. As the last card is thrown the signal 'up' is shouted.

These introspections and observations make it appear that the habits have developed just beyond the level of safe conscious supervision and yet were not sufficiently stable to stand the strain incident to the rate of speed at this stage. The break, then, is due in part to a direct effect of the conditions inherent at the initial stages of the inverse relation between consciousness and habit. When the habit, for any reason, failed, conscious-

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ness was unable to supply the needed movement and the whole performance became a blur. The second indirect cause of the break is due to the neuro-motor reflex aroused by the intense effort necessary to maintain, if not to excel, the level of the preceding record. Even holding their own produced a nervous and muscle tension to which the brief time and small practice had not inured them. In a former experiment, and as subject I observed, that the nervous strain was often too severe for the coördinating centers, resulting in confusion "hold-ups" and false movements.

The beginning of the *diminishing section* has been described as the end of the *middle section*. The section proper shows a marked uniformity within the limits of the practice, the gains steadily decreasing in amounts. There are four exceptions, J's already mentioned, and B's, C's and D's,—the latter only slightly. A description of the graphs expressing the results of P and U conditions follows here:

It will be recalled that the stimulus was given on the odd periods beginning with the third. In the case of C an exception had to be made and he received his stimulus on the even periods.

Graph 2. A's (P) curve shows rapid gains in the first section to 6P where the middle section begins. This section is of normal length, five periods, and shows relatively wide variations of increase. The final section begins at 10P and shows marked uniformity in the decrease of increments to the end of the practice. The sections of maximum and diminishing gains are comparatively uniform and in marked contrast to the middle section, thus emphasizing the storm and stress character of the latter. A's M. V. is 2.78 seconds per card. The rapid approach to the higher records is partly due to systematic methods of work and mental traits. The locations of the boxes were known at 5P and the order of the cards at 9P. This curve furnishes no evidence that the stimulus had any effect on the learning process.

D's curve (P), plan II, of the same graph conforms closely in its main outlines to K's (N) of graph 1. The variations of increase persisted throughout the practice more as a result of fatigue, and of nervous condition than as an effect of the stimulus,

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although the subject was inclined to think at times that the stimulus had a quieting, restful effect. Here the middle section from 7 to 11P is quite conspicuous and of normal length. D's M.V. is 3.25 seconds.

F's (U) curve is platted on the same graph with A's on account of their similarity in methods and in attitude of work, the variable factor being the stimulus, F was four periods longer in mapping the case, and it took him 33 more distributions to learn the order of the cards. The graphs show, however, that he reached the middle section on schedule time, 7P, one period later than A. F's initial record is considerably shorter than A's due doubtless to the latter's motor sluggishness. A excels F at 6P only, due perhaps to A's superior knowledge of the boxes at that time, having finished the map at the preceding period. The only considerable variations in the curve occur in the maximum section at 3 and 5P. There were no subjective disturbances reported from the stimulus effects during the 3P but during 5P both taste and noise proved a disturbance. He reported a feeling of relief when the noise ceased. The general form of the curve is not unlike I's (N) in the slight break at 11P at the end of the middle section and in the uniformity of the diminishing section. Consciousness and habits in his case maintained favorable relations. The growing habit was never seriously embarrassed with excess of knowledge of boxes and order of cards. F's M.V. is 2.46.

E's curve (graph 3). (U) shows in exaggerated form a break at 3P, 6 to 9 distributions, that appears at the same place in modified form in nearly all the curves, e.g., A, G, B (slightly), C, F, D, I and J. That is it occurs under normal, pleasant and unpleasant conditions. I first noticed it in a preliminary experiment where no stimulus was used. As 3P was the first time the stimulus was given, the fact calls for some notice. The chief cause, whatever it may be, was aggravated by giving the stimulus to E and G. Carbon disulphide was used for the first and only time with E and produced a very uncomfortable feeling. G's period came an hour later. The sulphide had been taken from the room in the meantime and asafoetida substituted, but

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the odor remained for several hours. The combined odors were obnoxious. G says, "At first I thought the odors would not disturb me, but during each distribution and while looking at the homely pictures a wave of disgust for the odors came over me." While such a stimulus undoubtedly accentuated the shock, it would be misleading to consider it the chief cause. The kinaesthetic habits at this stage were too weak to correct or even withstand the movements initiated by the false discriminations of box-labels. This false discrimination was characterized by sudden lunges toward a box only to discover a mistake. In the second place the conditions, so far as they relate to the movements, highly favored the action of retroactive inhibition. Time had been denied the associative tendencies to establish themselves and as Titchener observes (40): "Stimulus treads on the heel of stimulus. There is no impression of a pattern and no associations are formed." The kinaestheses of the 52 movements, to say nothing of the false ones destroy each other. The subject at such times may stand motionless or move the hand holding the card in a circle. This early experience appears common to all subjects regardless of conditions. The middle section of E's curve is embraced within four instead of five periods, 8P to 11P inclusive. This appears to be due to the rapidity with which he formed stable habits and thereby prevented the disturbing effect of consciousness on habit. E's M. V. was 2.31.

G excels E from 2 to 8P inclusive, but drops below at 9P and continues thus to the end. The former relation of the curves is explained on the ground that G completed place-memory of boxes and learned the order of the cards before E and the latter relation is due, perhaps, to E's superior control of attention, and to his capacity for readily forming stable habits. G's M. V. was 2.17.

B (P) claimed that the stimulus was slightly distracting at times. His initial record is the same as E's and excels those of the other subjects receiving a stimulus. But he drops behind at 2P and remains there throughout the practice. He did not reach the middle section until 12P and remained in it until 17

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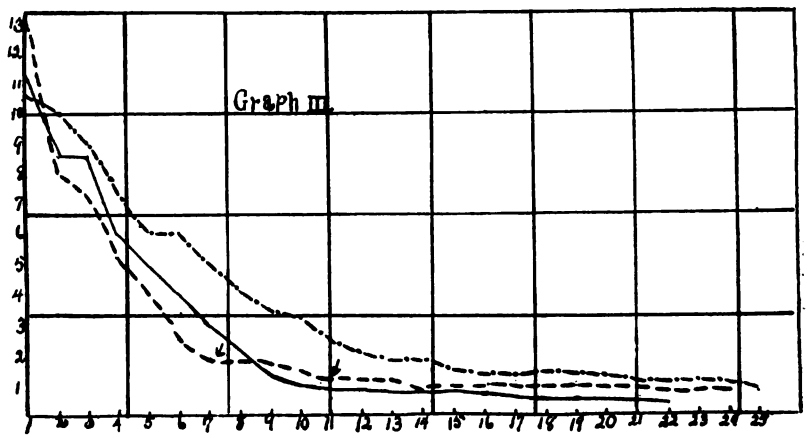
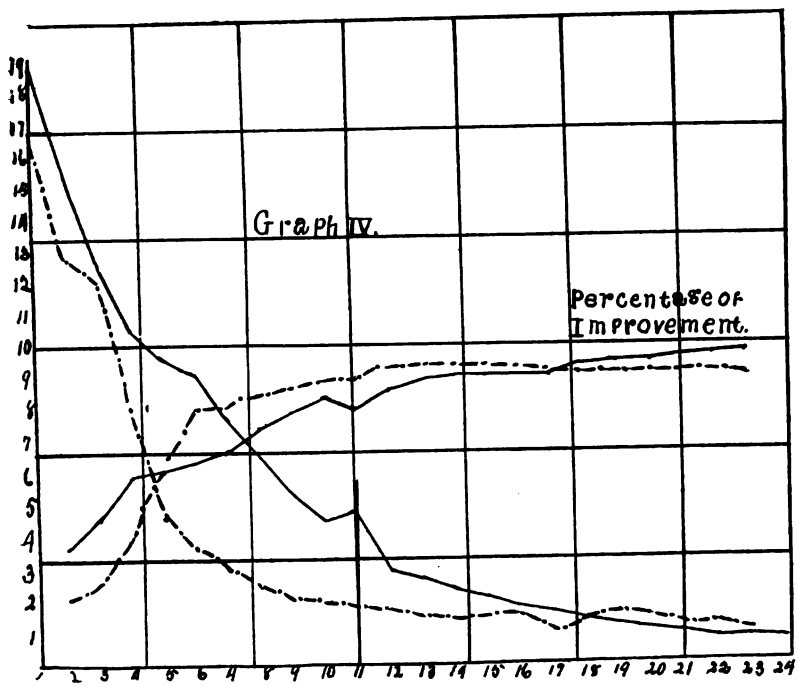


GRAPH III. STIMULUS CURVES

Unpleasant Stimulus: { Subject E = ———— / Subject G = — — — —
Pleasant Stimulus: Subject B = — - — - —



GRAPH IV. STIMULUS CURVES

Unpleasant Stimulus: Subject H = ————
Pleasant Stimulus: Subject C = — - — - —

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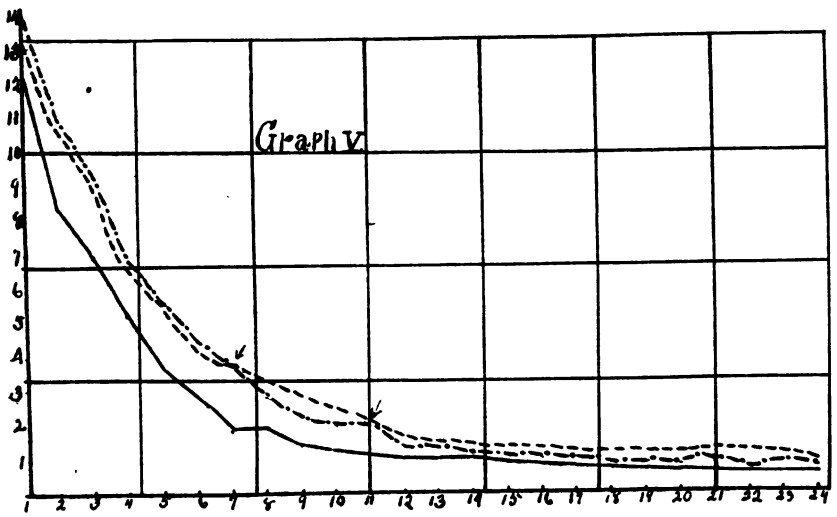
or 18P. He was slow in learning the order of the cards and in mapping the case, forming motor habits slowly. His M. V. was 2.56.

Graph 4. The principles thus far detailed in describing graphs 2 and 3 furnish a partial interpretation of the unusual curves of C (P) and of H (U) of this graph. C's strong place-memory enabled him to forge ahead to 6 P where an abrupt break occurs, owing to the unskilful use of his hands, to the lack of control of his body, to the shifting and unstable position of his feet



GRAPH V. COMBINED LEARNING CURVES

Curve for Unpleasant Stimulus: = — - — - — -
Curve for Pleasant Stimulus: = — — — — —
Curve for Normal Conditions: = ————————

while distributing, and to his failure in learning the order of the cards. He never passed beyond the earlier part of the conscious group stage in synthesizing movements. The diminishing section of his curve shows a bare passage beyond the middle section. His M. V. was 3.15.

H (U) was handicapped throughout by a poor place-memory, and to some extent by his failure to learn the order of the cards. The plan of the work proved a stumbling block and at times he suffered a complete relapse. He claimed that the stimulus was

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a positive hindrance to his learning. It was suggested that the relapse at 11P was due to the interval of no practice on account of the Christmas vacation. Perhaps so, but it is an exception, since the other subjects show a strong normal gain at this period, rather indicating an aid from the brief vacation. He passed C at the 18P. This is undoubtedly due to C's failure to improve the use of his hands. H's M.V. is 4.08.

Summary for charts. (1) The major influences on the variations of the curves from the normal are differences of individuality and differences in methods of work.

(2) No positive statement can be made concerning the influence of the pleasant stimulus, but the evidence points to no effect. The unpleasant stimulus tended to create a nervous tension and to serve as a spur or stimulus to greater effort, especially toward the completion of the second and in the final section. Rose (35) found a similar effect on reaction time from unpleasant sensation of varying intensities, regardless of the individual and the manner of reaction.

(3) The size of the increments toward the close of the practice depended largely on a skilful use of minor aids and devices, e.g., manner of using the left hand and thumb.

(4) The rapidity of reaching and passing the middle section depended upon an active and energetic manner of conceiving and solving the conditions of the problem. The comparative lengths of these three sections are expressed in the several graphs.

(5) Errors, dropped cards and inaccuracies increase with higher speed up to a certain limit and then, with the more rapid rates, decrease.

3. Difficult cards and critical movements. In describing the location of the boxes it was shown that some of the more difficult boxes were associated with cards occurring in special sequences and delivered by movements of leftward direction. There is no evidence that the subjects, save F and J were aware of such cards and movements during the mapping stage. But as the movements began to integrate both difficult cards and critical movements attracted attention. Of course the familiar, easy card and the ready, free, fluent movement are pertinent facts

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to our main problem. But here interest is attached to those cards and movements that proved a positive check to speed throughout the major part of the practice. We enumerate them here, postponing an explanation to section C.

The most troublesome cards were the spades, going to diamond boxes (plan I). Ranked in order of decreasing difficulty they run as follows 2 of S, K of S, 9 of S, 3 of S, 4 of S, 10 of S. Hearts ranked next to Spades and showed the following order of decreasing difficulty: 5 of H, 3 of H, 9 of H, 10 of H and 7 of H, the latter being comparatively easy; Diamonds came third, in the order: 2 of D, K of D, A of D and 6 of D. (Diamonds were thrown to club boxes). Some of the more difficult clubs were 5 of C and 2 of C. These cards began to attract the subjects' attention by reason of their difficulty from the twentieth distribution. The critical movements, not necessarily difficult, originate almost exclusively with the club boxes. This arose from the fact that these boxes were the starting points for two divergent movements; and the difference in the setting of the two movements, starting from the same point, but belonging to different sequences, created confusion, e.g., the two movements that delivered the A of C and the A of D respectively start from the J of C. The former occurs in the second and the latter in the third thirteen (plan I, see also diagram 2).

The following club boxes were regarded as confusion points:

J of C — A of C, A of D'; Q of C — 5 of C, 5 of D'; 3 of C — 10 of C, 10 of D'; 8 of C — 2 of C, 2 of D';

6 of C — 4 of C, 4 of D'; K of C — 9 of D, 9 of C'; 10 of C — 8 of D, 8 of C

An inspection of diagram 3 shows all the essential features and factors creating "critical points" and "critical movements" and shows how inhibitions of movements may arise and persist in the acquisition of skill. The confusion at the 6 of C is aggravated in both cases from the fact that the movement to the 6 of C follows the difficult movement to the 5 of H in the one case,

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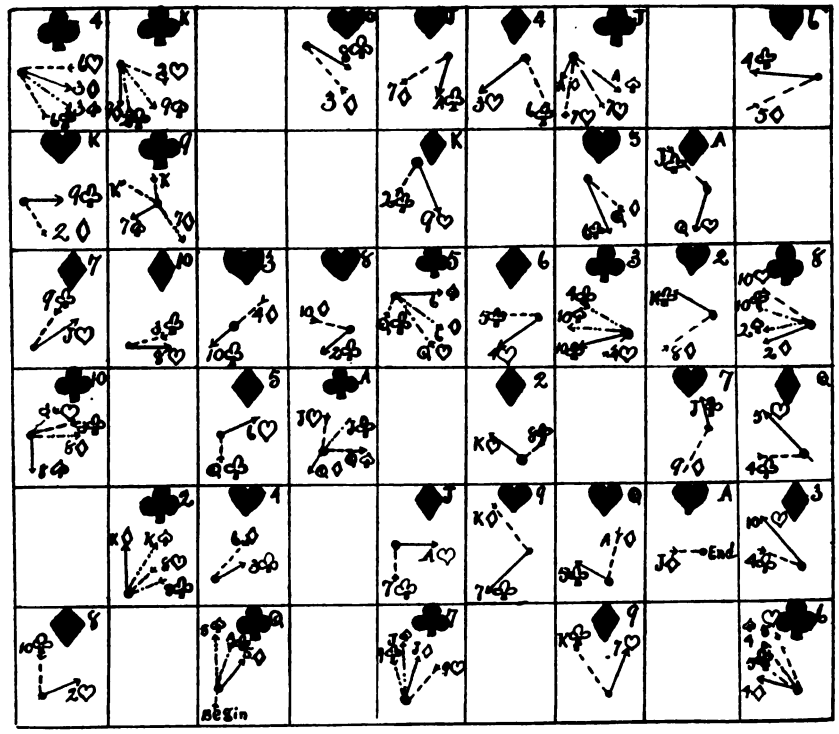

DIAGRAM 3

First movement to a box = → — — — —
First movement away from box = ——————→
Second movement to a box = →- — - — - —
Second movement away from box = — - - — - - →

Name of card at arrows pointing toward center of box indicates the box from which the movement was made, and the name of card at arrows pointing away from a box indicates the next card to be delivered.

The following club boxes were regarded as confusion points:—8 of C, K of C, 10 of C, 6 of C, J of C, Q of C. The direction of the arrows pointing away from these suggests a reason for the confusion of movements.

Plan of distribution

Order of suits: Club, diamond, spade, heart.

Order of stacking the cards: Q, 5, 6, 4, 3, 10, 8, 2, K, 9, 7, J, A.

Order of distribution: Spades to Diamonds, Diamonds to Clubs and Clubs and Hearts to their own boxes.

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and to the 5 of C in the other. The movement from the J of H was confused by some with the movement from the J of C. The subject found that these confusion points could be overcome, provided the movement was recognized in its proper setting and time relations. Until the distribution was well mechanized, skill in passing the critical points depended upon the readiness to anticipate and discriminate between the rival movements.

4. Feeling tone accompanying the distribution. a. Artificially created.—The first reports of the subjects, contrary to anticipation, uniformly show a comparatively slight disturbance from the stimulus. Even the unpleasant stimulus was mildly amusing, to be forgotten as soon as the distribution began. Such reports as "The stimulus did not amount to anything," "I forgot about the stimulus," or "It does not disturb me in any way," were often made. To sum up the matter for stimulus effects:

(1) P stimulus is for the most part neutral, though there is some evidence that it lessened the number of errors, of dropped cards and of hold-ups.

(2) The stimulus effects were more noticeable in periods of high records, and in the case of the U stimulus increased the number of hold-ups.

(3) The U stimulus served both to increase and decrease the distributing rates, the former by adding momentum to the smoothly running movements and the latter by prolonging the relapsed condition of a movement or of the place memory for a box. "If I stumble the stimulus tends to keep me down, but as long as I keep up and going it increases the speed."

(4) The subjects who took stimuli, whether P or U, made more errors in the non-stimulus periods than in those in which stimulus was given, and the subjects taking P stimuli dropped more cards and had more hold-ups in non-stimulus periods, while subjects taking U stimuli dropped more cards and had more hold-ups during stimulus periods. This shows for the most part that the subjects did better during the stimulus periods, a slight exception being with subjects taking U stimuli. See table 5 in this connection.

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TABLE 5

Showing number errors, dropped cards and holdups in relation to stimulus

SUBJECT	ERRORS				DROPPED CARDS				HOLDUPS			
	With stimulus		Without stimulus		With stimulus		Without stimulus		With stimulus		Without stimulus	
	Number of errors	Per cent	Number of errors	Per cent	Number of dropped	Per cent	Number of dropped	Per cent	Number of holdups	Per cent	Number of holdups	Per cent
Pleasant Stimulus												
A.....	2	14.3	12	85.7	1	14.3	6	85.7	24	48.2	25	51.8
C.....	5	41.7	7	58.3	22	44.9	27	55.1	9	39.1	14	60.9
D.....	30	41.7	42	58.3	8	38.1	13	61.9	12	54.5	10	45.5
B.....	12	54.5	10	45.5	11	55.0	9	45.0	22	53.7	19	46.3
Total.....	49	40.8	71	59.2	42	43.3	55	56.7	67	49.6	68	50.4
Average..	12.3	41.0	17.7	59.0	10.5	43.4	13.7	56.6	16.8	49.4	17	50.6
Unpleasant stimulus												
G.....	44	40.3	65	59.7	29	30.5	66	69.5	50	62.5	30	37.5
E.....	22	44.9	27	55.1	46	51.7	43	48.3	6	75.0	2	25.0
F.....	43	47.2	48	52.8	99	58.9	69	41.1	35	59.3	24	40.7
H.....	6	37.5	10	62.5	13	81.3	3	18.7	34	43.0	45	57.0
Total.....	115	43.4	150	56.6	187	50.8	181	49.2	125	50.8	101	49.2
Average..	28.8	37.7	47.5	62.3	46.7	50.8	45.3	49.2	31.3	55.3	25.3	44.7
Normal												
I.....			24				52				4	
J.....			54				26				53	
K.....			66				73				40	
Total.....			144				151				97	
Average..			38				50.3				32.3	

This table shows percentages of errors, dropped cards and holdups, with and without stimulus, e.g., A made 14 errors; 2, or 14.3 per cent, were made when the stimulus was used, 12, or 85.7 per cent, when it was absent. 40.8 per cent of the total of errors was made with stimulus, 59.2 per cent without.

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b. Feeling tone excited by the distributing process. The introspective reports on the feeling tones excited by the distributing process both confirm and supplement considerably some earlier observations on the same questions. Kline and Owens (25) observed that the practice produced a "wide range of affective tones and gave direct evidence bearing on the formation of a number of the more common attitudes and feelings;" such as bewilderment, confusion, distrust, anxiety, dread, hesitation, strain and tension, humiliation, chagrin, confidence, satisfaction, elation etc. These feelings and attitudes were prominent in the present study and were related to the speed records both as cause and effect. There appears to be no doubt that they both lowered and heightened the records. But did they at the same time *hinder* or *facilitate* learning, or were they neutral? Did the feeling tones aroused by the kinaesthesis involved in the practice and by the inherent conditions of the plan of the experiment hinder or favor the learning?

With the passing of the interest of novelty, of the early optimism, of the satisfaction at the "lucky hits," there set in a confused, bewildered, dazed feeling accompanied with eye-strain, lurching of the whole body from side to side and to and fro, thrusting, instead of lightly tossing, the card in the box after a long search. Here, too, were attempts to storm or rush the work only to recognize its futility. Then most of the subjects settled down to appreciate the work more definitely by ascertaining vantage points and by devising plans and methods of attack detailed on former pages. By such systematic procedure the more formidable inhibitions, the more serious and permanent confusions began to impress the subjects so that they were able to report the feelings and attitudes involved. The reports show that the feelings center about three sorts of activities, which have already been mentioned, but call for critical notice here.

First. Feeling tones grew out of the two substitutions, and since they occurred in immediate succession, their effect was cumulative. The order of the suit it will be recalled, ran C, D, S, H and the substitutions fell on the inside cards, diamonds being thrown to clubs' boxes and spades to diamonds, so that the

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order of the boxes visited in delivering a suit were C, C, D and H. The evidence, to be submitted below, shows that even the distribution of the hearts was affected by the cumulative effects of the double substitution. A study of the reports on the feeling qualities of movements shows that the number of U (marked— in table 3) movements made in delivering cards requiring substitution exceeded the number of unpleasant movements with cards not requiring substitution. A numerical comparison of the averages of the percentages of the U reports of plans I and II, table 3, including the movements listed under both "series" and "individual," shows 25.7 per cent and 25.5 per cent for these two sets of reports unaccompanied by substitution of suit, and 16.7 per cent and 18.8 per cent for the two sets ("series" and "individual") not involving substitution.

While these results offer ample evidence of the burden involved by substitution and caused the subjects to regard such movements as unpleasant, yet the unpleasantness was not due to the *movements as such.*

Second. Feeling tones grew out of the confusion arising from two divergent movements. The conditions of plan I required two diverging movements from club boxes to club and to diamond boxes, respectively. Diagram 3 shows 13 such diverging points, already described as "critical" points. A critical inspection of this diagram shows the problems and difficulties encountered. Also the movement from club to club box carrying either a diamond or a club was often checked at the second visit by finding a club or a diamond already there. This proved disconcerting.

At the 5P, G points out that at that time he knew more diamonds than heart boxes, but that an unknown heart box was easier to locate than an unknown diamond box.

Other subjects of plan I found the same thing. Some made exceptions of 7 of H, 5 of H and 3 of H. Comparatively hearts gave little or no trouble at first, but as the movements began to form orderly sequences, the cumulative effects of spade to diamond and diamond to club already described began to operate adversely on hearts. I interpret this as a delicate instance of the effect of feeling tone postponing the learning process.

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Perhaps at this point evidence should be given indicating cards that are easy and their relation to feeling. Specific evidence, although indirect, was secured by having the cards delivered unstacked and again stacked backwards. Subjects J, B and to some extent F found the same cards giving trouble in both the usual and these unusual ways of distribution, but A, E, G, H and I report that those cards, boxes and movements that were well mechanized in the usual distribution gave more or less trouble in the unstacked and the backward order. The cards that were thus brought back to consciousness in an unfavorable light were, J of H, 8 of H, 6 of H, 4 of H, 2 of H, 7 of C, 6 of C, J of C, A of C, 9 of C, J of S, 7 of S, 6 of S, 5 of S, Q of D, 10 of D. These cards go to boxes that were among the first to be mapped. E says, "I had to repeat the label of the box receiving the card; due to interference of habit. Cards that were easy before are difficult now and vice versa." Subject H reports, "These cards gave no trouble in stacked order. They had become so well mechanized that when conscious had to take account of them they became difficult. I am not accustomed to *see* where I throw, but to *feel* where I throw." G says, "Interference as backwards was not felt especially as motor interference, the sight of a card would call up its antecedent, but not its subsequent. I broke down when this did not occur." These some 16 or more cards caused the fewest "actual" and "memory" hold-ups, and as I shall show later most of them belong to or were delivered by pleasant movements.

Third. Feeling tones were excited by movements by virtue of the ease or difficulty with which they were executed. Here I found three separable conditions (a) The feeling tone of an arm movement depends upon its direction, (b) upon its length, (c) upon its combination with other movements. There is some evidence to the effect that the feeling tone of a movement depends upon whether or not it is known, i.e., unknown movements are unpleasant, known are pleasant. But my records fail to establish the validity of the view, besides a very little experimentation shows that the direction of a point may be known and the actual movement remain unpleasant.

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To these three inherent conditions should be added an external one occurring too frequently to be ignored. If the subject made an exceptionally good record and attempted to excel it in the one immediately following it, the second record often fell below the average for that period, e.g., Subject K at the 12P distributed the pack in 1 minute 5 seconds and in the next distribution the time was lengthened to 1 minute 20 seconds, a loss of 15 seconds. This subject sustained such losses in six different records from 12P to 24P or in 50 per cent of the 12 periods. All subjects had somewhat similar experience. B, J and H suffered losses from this cause in from 60 to 70 per cent of the records beyond 10P. The loss might occur in any distribution after the second, and never before the third. Both I and E knew how to profit by the warming up of 1D and 2D and so how to avoid the nervous "stir up" in fast records by not forcing the habit in the subsequent ones. They had few hold ups (table 5). The subjects always volunteered to give this experience when it occurred and their accounts checked with the time record. The feelings mentioned as resulting were distrust in themselves, confusion and exasperation.

Summary of the sources of feeling tones. Poor and good records, minor blunders and achievements, substitutions and their consequences (difficult cards) and the nature of the movements were the common sources of feelings. Of the cards, the reports show that the spades were the most active, then the diamonds, followed by the hearts with here and there a club. There remains to be critically noticed the relation between movements and feeling tones.

c. Movements and feeling tones. The K-9-7-J system of movements occurring in the second thirteen tended to collide with a similar system in the third thirteen. The two systems are shown in their relative positions in diagram 4. All subjects having plan I experienced decided feelings when executing the movements of either system. The diagram shows that the two systems have many similarities, same numbered cards, converging to the same point, J of C, and few small differences. One movement was radically different and the suits of course

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were different. The reports show the following points concerning the feeling tone and arm movements:

First. Long movements tended to lose their agreeableness with increased rate and short movements in certain combinations, e.g., ballistic, grew in favor. Consecutive movements in one and the same straight line were unpleasant because they frustrated habits of a right and left or an up and down movement. "Balanced movements are pleasant, e.g., if a right movement is followed by an equal left movement the effect is pleasant and the movement is remembered."

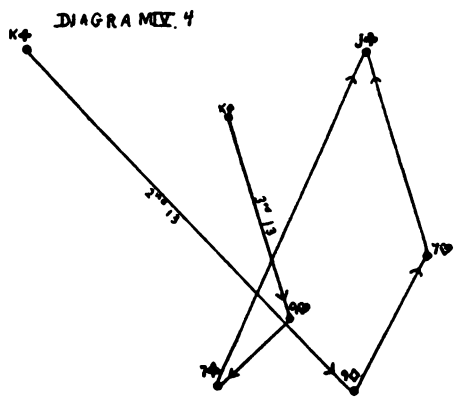

DIAGRAM 4. Showing similar movements in distributing like numbered cards but of different suits; a confusing condition.

Second. The feeling tone of a movement made with the right arm under the conditions of the distribution depends upon its direction. The order of preference appears to be as follows (1) horizontal to the right (2) right oblique up, (3) right oblique down, (4) vertical down, (5) vertical up, (6) horizontal to the left, (7) left oblique *down*, (8) left oblique *up*. While the reports differ in the preference for right oblique up and right oblique down, the majority favored the oblique. Subject F who was left-handed, gave a different order of preference, but the judgments did not appear sufficiently consistent for record.

Third. Perhaps the most common and yet the most elusive factor in determining the feeling tone of a movement was its

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setting and combination with other movements. A movement disliked for its length in one stage of practice became agreeable at another and vice versa. But here, too, certain movements produced the same feeling tone, despite the synthesis formed with other movements, e.g., the movement from 4 of D to 3 of H was continually reported as foreshortened and unpleasant by A, B, C, and G. A reports that it prevented the formation of a balanced movement. It was one of two consecutive movements made in one and the same straight line to the left of the median plane of the body, and also it frustrated the formation of a habit of a right and left movement. There were four of these foreshortened movements. Two of them were finally fused into a *one-two-three* rhythm, and thereby became pleasant.

Fourth. The rapidity with which long individual movements and synthesized movements were made had a direct influence on the feeling tone of the movement. Broad sweeps at moderate rates were pleasant, but at high speed they were unpleasant. The reasons assigned were (1) the difficulty of control, or "posterior correction," according to Woodworth, in the interest of precision, and (2) the sudden angular lurching of the body. Woodworth (43) observes "Slow movements are, for the most part, depressing, just as rapid movements are distinctly stimulating. Thus a slow movement, by its emotional tone, tends to make the second movement still slower and a rapid movement acts as a stimulant, so a slow soft music is apt to be rendered still slower by a performer, and a rapid movement unduly accelerated." The subjects reported tingling sensations, feelings of lightness and exhilaration, after a distribution of free, fluent, moderately rapid movements. Here, as in length, the optimal or moderate speed could not well be determined, and it doubtless shifted with the stages of practice. Direction, length, rate and setting of a movement are determinants of its feeling quality, each requiring special investigation.

Fifth. Several of the subjects, G, J, D and F reported that the feeling quality of a movement depended on the nature of its result and the service it rendered, e.g., G says, 13P., "Some of the long movements are pleasurable because they were easy to

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learn, they aided in fixing the boxes." F reported that movements easily visualized were pleasant, otherwise unpleasant. And in making a report on the feeling tone of mimetic movements before the case observed that the movements well known were pleasant and those unknown otherwise, and that being known was not conditioned in any way on the feeling quality arising out of the movement, but rather on its results. Surely results are functions of feelings, and no doubt often obscure those arising from other sources, especially those from kinaesthesis; just as the pleasant affective tone of recognition may be obscured by the feelings accompanying the object of recognition. I am fully persuaded that these subjects either mistook and confused the feeling quality of the movement for that accompanying the result, or else the feeling tone of the latter completely obscured that of the former. The proof that the feeling tone of a movement is independent of any cognitive element is seen in the reports of A, E, and I who knew the location of the boxes and had synthesized many of the movements, yet were still sensitive to their agreeable, disagreeable, tense and relaxed qualities. The relation between the direction of a movement and its readiness to serve skilful purposes is partially conditioned on the nature of the feeling quality involved, as we shall see.

C. Rate of learning and feeling tone correlated

1. Inhibitions (*hold ups*). One of the most striking and persistent experiences connected with the distribution was the fact of inhibition. Its chief sources lay (1) in the plan of the work governing the substitution of the suits, (2) in the manner of stacking the cards, particularly as to the order of the suit, and (3) in the manner of labeling the boxes, the details of which have already been described in "Plans and procedure." This arrangement facilitated the elimination of superfluous movements to a minimum, if not to a vanishing point and at the same time developed a system of more or less rigid motor habits. For a time practice accentuated the constituent movements and favored a description of their conscious aspect. In this conscious

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aspect inhibition became a common event and more and more localized with respect to certain movements. Even in the *vague motor attitude* of the synthetic stage of movements the inhibitions of a localized form began to appear and continued throughout the practice even in the automatic movements, where they occurred with greater impact if with less frequency.

But the strictly motor inhibitions were preceded and accompanied by a perceptual or a cognitive kind, so-called, growing out of the task of "establishing card." The nature of these inhibitions is described in some detail in the discussion submitted above under the headings, "Learning the plan of distribution" and "The learning curves" and finally in "Feeling tone excited by the distributing process." Interest here is not in the inhibitions involved within the plan itself, but in their diffusive effect

TABLE 6

	UNACCOMPANIED BY SUBSTITUTION		ACCOMPANIED BY SUBSTITUTION	
	Felt in "a series"	Felt in "Isolation"	Felt in "a series"	Felt in "Isolation"
	per cent	*per cent*	*per cent*	*per cent*
Pleasant	72	69.5	62.5	62.5
Unpleasant	16	18.5	25.5	25

on the movements. A numerical comparison was made between the unpleasant (−) reports of the 26 movements "unaccompanied" by substitution of card and those "accompanied," showing a decided effect of the latter process on the unpleasant judgments. The pleasant (+) reports show a similar effect as might be expected, i.e., movements unaccompanied by substitution of card show a higher percentage of (+) reports. Percentages of both kinds of reports are shown in the accompanying table 6, abridged from table 3, plans I and II.

The plan or "aufgabe"[7] through practice becomes associated with the kinaesthetic processes of execution and facilitates or

[7] The description here of the function of the 'aufgabe' apparently coincides with that given by Professor Washburn in her paper on "The Aufgabe and Intellectual Inefficiency," an abstract of which has just come to hand. Proc. Amer. Psych. Assoc., December, 1913; Psy. Bul., vol. xi, pp. 41, 42, 1914.

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hinders the progress of the latter even though the plan itself has dropped out of consciousness. The explanation of how either the facilitating or inhibiting effect is accomplished doubtless grounds on the nature of the operation or responses of the nervous mechanism involved. Here, however, the interest centers in the effect of the feeling tone aroused by the conflicting cognitive processes on the immediately subsequent movement. The evidence points unmistakably to an adverse influence of the feeling tone excited by the suit substitution on the movement in question. I see no other explanation for the decidedly high percentages of U (−) reports of movements accompanied by substitution. The introspective evidence of the feeling tone aroused by the substitution process is clear enough while the learning of the plan was on the conscious plane, but the subjects do not report that it consciously affected the movement and there is no apparent reason why it should. It was only after the plan had become unconscious and the essential movements were being mechanized that the *affective* relation between suit substitution and movement was discovered. But we are not yet in a position to say whether or not the feeling tone thus fused with the kinaesthesis favored or hindered its integrating into a skilful act.

The causes for the strictly motor inhibitions have been anticipated in the discussion of critical movements (learning curve) and of difficult movements (feeling tone excited by the distribution process). In the former case it was seen that the inhibiting tendency of two divergent movements was inversely as the size of the diverging angle. The feeling tone here involved was a menace to high speed and perhaps to learning, but it was the effect, in the beginning at least, and not the cause of the conflict between the kinaestheses of the two movements, and besides the feeling did not grow out of the movement per se. But in the latter, the difficult movements, the kinaestheses were accompanied by feeling qualities which proved a handicap to their readiness of habit forming. If we assume that the better learned movement sare the more accurate (fewest errors), the more rapid (the least hold ups) and the better controlled (fewest dropped

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cards), then it should be possible to ascertain the degree of correlation between their feeling qualities on the one hand and each of these characteristics of skill on the other. Or that the poorer learned movements are less accurate (most errors), more hesitating (most hold-ups) and less controlled (most dropped cards), then it should be possible to ascertain the degree of correlation between the feeling qualities of such movements and each of these defective characteristics of skill.

The statistical facts forming the basis of the correlation were taken from a larger table (not given here), of which table 3 is a summary. It should be recalled that the movements on which the reports of the feeling qualities were given were made before the case in mimetic fashion, card in hand, without distributing the pack, giving the judgment as each movement was executed. Each of the 52 movements being judged for its feeling qualities 47 times in relation to the system of movements in which it occurred, and 50 times as an individual movement, was grouped according to its direction. The percentage of any judgment quality was calculated on the total number of reports for that particular group of directions. For example, in the group of oblique movements, right up, of which there were five, the totals are: Pleasant (+) 168, unpleasant (−) 11, neutral (0) 43, and (t) 13, making a grand total of 235 feeling-judgments of movements integrated into a "series," or 71.5, 4.7, 18.3, and 5.5 per cent respectively. The subjects repeated these judgments for each of the 52 movements from 7 to 9 different times. In the case of the "memory report" of feeling, (see p. 267) the judgments of each subject were taken at two different times only.

The mutation of the feeling qualities of the movements with the increase of skill is submitted as a check on the reliability of the reports on such qualities. For this purpose the reports were arranged in a time series according to weeks and to subjects. A study of this table showed: First, that very few tense (t) feeling qualities were reported on the "isolated" movements, this quality being confined almost entirely to the movement in its practiced setting, and that the tense qualities grow *less* with the acquisition of skill. Second, contrary to expectation, that

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the neutral (0) reports decreased rather than increased with practice in all cases except with subject J. Third, that the U (−) qualities exhibit no uniform tendency, with some, as A, E, F, I, J, and D, the U reports ("series") tend to decrease with practice; but with K, B, C, and G the U reports increase slightly. Fourth, that the P reports ("series") increased with the majority of the subjects, viz., with A, E, F, G, I, and D.

The steady decline in the neutral(0) judgments, the relatively small increase in P feeling-judgments and the absence of any marked mutation on the whole were not anticipated. If the feeling judgments had been made earlier in the practice, before the movements had become so well integrated, the mutation of feelings would doubtless have been greater.

Here attention centers on the correlation between the affective character of movements and the number of *inhibitions* (hold-ups) involved. In table 7 arrays are made in ascending order of P and U reports, respectively, on the feeling qualities of 51 movements regardless of substitution of suit and of direction. To the right of the feeling arrays are the columns of "hold-ups" and errors. A mere inspection of the array of P reports and "hold-ups" shows minus or inverse correlation, or the more pleasant the movements the less frequent the inhibitions. The correlation computed by Pearson's "Product-moments" method gave $r = -0.54$ with a P. E. of 0.0678. The array of the U reports and corresponding "hold-ups" in the same table suggest a positive correlation; the inhibitions increase as the number of U reports of the movements. The coefficient, $r = 0.48$ with a P. E. or 0.072. The correlation of errors with their respective P and U reports was so slight as to be negligible, the P giving a minus and U a positive.

Since the substitution of suit excited U feelings and imposed a handicap on the movements, it became necessary to consider those movements unaccompanied by substitution. Accordingly "hold-ups" (inhibitions) were correlated with the respective P and U feeling qualities of 25 movements unaffected by substitution (the exceptions have already been noted).. The P movements, free from substitution, gave a *higher* negative correlation,

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TABLE 7

Correlation between P movements and holdups and errors and between U movements and holdups and errors

NUMBER OF MOVEMENT	DIRECTION OF MOVEMENT	ARRAY OF P REPORTS ON MOVEMENTS	HOLDUPS	ERRORS	DIRECTION OF MOVEMENTS	ARRAY OF U REPORTS ON MOVEMENTS	HOLDUPS	ERRORS
1	U	12	18	7	R	0	0	3
2	LD	17	26	7	R	0	1	4
3	LD	19	32	5	RU	0	2	5
4	LD	19	7	7	RU	0	3	6
5	LU	19	25	3	RU	0	9	13
6	LU	19	10	7	RD	0	2	1
7	L	20	10	2	U	1	3	2
8	LU	20	36	13	RU	1	3	3
9	LU	20	5	6	LD	1	7	12
10	LD	21	7	9	RU	2	7	2
11	RU	21	26	3	RD	2	27	16
12	LU	22	14	8	RD	2	2	21
13	LD	22	49	23	R	3	4	3
14	RD	22	22	9	R	3	3	8
15	LD	26	3	4	RU	3	5	3
16	LD	26	5	4	D	4	6	2
17	LD	26	14	3	LD	4	6	23
18	LD	26	7	3	LD	4	8	12
19	D	27	1	2	LD	5	11	2
20	LU	27	9	10	RD	5	7	3
21	LU	27	13	20	U	6	8	7
22	LD	27	8	12	LD	6	2	3
23	R	28	5	11	LU	7	22	9
24	L	28	5	1	RD	7	13	10
25	RU	29	8	4	RU	8	8	4
26	RU	30	5	3	RD	9	2	5
27	RU	30	6	9	RD	9	2	5
28	RU	30	7	2	RU	9	6	9
29	RD	30	27	16	L	9	5	1
30	RU	31	2	5	RD	10	13	5
31	RD	31	2	5	D	11	1	2
32	RD	31	19	6	L	11	12	2
33	LD	31	2	3	LU	11	36	13
34	RD	32	13	5	LU	11	9	20
35	RD	32	11	6	LD	11	7	9
36	LU	34	3	3	R	12	5	11
37	D	34	6	2	RD	12	19	6
38	RU	34	3	3	LU	13	3	3
39	RD	35	2	5	LD	13	3	4
40	U	36	8	7	LD	13	5	4
41	R	37	3	8	LD	13	14	3

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TABLE 7—*Continued*

NUMBER OF MOVEMENT	DIRECTION OF MOVEMENT	ARRAY OF P REPORTS ON MOVEMENTS	HOLDUPS	ERRORS	DIRECTION OF MOVEMENT	ARRAY OF U REPORTS ON MOVEMENTS	HOLDUPS	ERRORS
42	LD	37	6	23	RU	13	26	3
43	RD	39	2	21	LD	15	49	23
44	R	41	4	3	U	16	18	11
45	R	43	0	3	LU	16	5	6
46	RU	43	3	6	LU	18	25.	3
47	RU	44	9	13	LD	19	32	5
48	LD	44	3	2	LD	20	7	7
49	RD	46	2	1	LU	20	14	8
50	U	46	7	12	LU	21	10	7
51	R	47	1	4	LD	21	26	7

The correlation by the Pearson coefficient between P movements and holdups r = −0.54 and PE = 0.0678.

The correlation between U movements and holdups r = +0.48 and PE = 0.072.

The correlation between the U and P movements and errors is negligible.

U = up; D = down; LU = left up; LD = left down; RU = right up; RD = right down; R = right; L = left.

The number of Right and Left movements is equal in each of the U and P arrays.

These movements required substitution of cards.

r = −0.56, and the U movements, now less unpleasant, gave a *lower* positive correlation, r = +0.40 (see table 8). The errors in this table also gave a negligible correlation; they are caused by several other factors aside from U feelings of a movement.

The negative correlation between P feeling qualities of a movement and inhibitions and the positive correlation between U feeling qualities and inhibitions are strong evidence that feelings excited by movement exert an appreciable influence on its habit forming capacity: hinder if the feeling quality is unpleasant, facilitate if the quality is pleasant.

The facts go still farther and show that the feeling qualities are dependent upon certain definite features of the movements.

2. Feeling tone and direction of movement. Attention has been directed repeatedly to a decided relationship existing between unpleasant feelings and leftward movements on the one hand, and pleasant feelings and rightward on the other. To gain a numerical expression of the relationships a classification of the reports on

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the feelings of movements was made according to the cardinal and semicardinal directions. The results are given in table 3 including the feeling judgments for movements considered in a "series" and as "isolated" for both substitution and non-substitution of suit. A typical rather than an exhaustive comparison of the results is made.

TABLE 8

Correlation without substitution of suit between P movements and holdups and errors and between U movements and holdups and errors

NUMBER OF MOVEMENT	DIRECTION OF MOVEMENT	ARRAY OF P REPORTS ON MOVEMENTS	HOLDUPS	ERRORS	DIRECTION OF MOVEMENT	ARRAY OF U REPORTS ON MOVEMENTS	HOLDUPS	ERRORS
1	LD	19	7	7	R	0	1	4
2	LU	19	10	7	RU	0	2	5
3	LU	19	25	3	RU	0	9	13
4	LD	19	32	5	RU	1	3	3
5	LU	20	36	13	LD	1	3	2
6	LU	20	5	6	RD	2	27	16
7	LD	26	14	3	RU	2	7	2
8	LD	23	7	3	R	3	3	8
9	LU	27	9	20	R	3	4	3
10	L	28	5	1	LD	4	6	23
11	RU	29	8	4	LD	5	7	3
12	RU	30	7	2	RD	5	11	6
13	RD	30	27	16	LD	6	2	3
14	LD	31	2	3	RU	8	8	4
15	RU	31	2	5	RD	9	2	5
16	RD	32	11	6	L	9	5	1
17	RU	34	3	3	LU	11	36	13
18	LU	34	3	3	LU	11	9	20
19	RD	35	2	5	LU	13	3	3
20	R	37	2	8	LD	13	14	3
21	LD	37	6	23	LU	16	5	6
22	R	41	4	3	LU	18	25	3
23	RU	44	9	13	LD	19	32	5
24	LD	44	3	2	LD	20	10	7
25	R	47	1	4	LU	21	10	7

Correlation between P movements and holdups is: r = −0.56, PE = 0.0185.

Only a slight correlation exists between P movements and errors.

Correlation between U movements and holdups is: r = +0.41, with PE = 0.0248.

No correlations between U movements and errors.

The legend is the same as in table 6.

The movements in both the P and U arrays are equal with 44 per cent to the Right and 56 per cent to the Left in each.

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If we consider only the movements of a "series" in plan I of the table we find that the P reports (+) for the rightward and leftward movements are, respectively, 70 per cent and 53 per cent, and that the U reports (−) for the rightward and leftward movements are 10 per cent and 27 per cent, respectively. Further the P (+) reports for right and left horizontal movements are respectively 82 per cent and 51.5 per cent while the U (−) reports show 8.5 per cent for the horizontal right and 24 per cent for the horizontal left per cent. Here then is a decided preference for the rightward movement. And it should be borne in mind that when the feeling qualities of these movements were reported neither the experimenter nor the subjects were considering their directions. We were concerned with the movements as they occurred in a system, whether or not they involved substitution of suit, etc.

An inspection of the direction movements shown in table 6 by letters reveals the same principle in a more evident way. Prevailing leftward movements are associated with an increase in inhibitions and U feelings and prevailing rightward movements are connected with a decrease in inhibitions and with the higher numbers of P feelings. In table 7 "P Reports" the twenty-sixth movement of the array is the median. If the vertical movements above and below the median be disregarded, there remains above it 19 or 90 per cent of the 21 movements that are leftward, and below it 18 or 81 per cent of the 22 movements that are rightward. The movements in the array of the "U Reports" bear practically the same ratios as to direction.

3. Errors, dropped cards, and direction of movement. By far the greater part of the errors were mechanical, consisting of cards thrown into boxes adjacent to the correct box. Next in order of frequency were errors of confusion. These consisted in mistaking a spade box for a club, or a heart for a diamond, or vice versa. The fewest errors were those of "motor attraction" caused by the similarity of direction and of extent between two or more movements occurring in different sequences. For example, 7 of D, which occurred in the fourth 13, was frequently thrown to 7 of D owing to the fact that the 7 of S in the K-9-7 group of the

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first 13 was thrown, according to plan, to 7 D. The errors of "sensory attraction," socalled, more numerous than those of the motor, were made by throwing a diamond to a diamond box of like number. Errors of omission were seldom made. They occurred in the higher rates of speed and especially with those movements that threw cards to boxes occupying the vertices of a triangular form of integrated movements and, in the effort to speed up, the box at the vertex was often omitted.

But since a majority of the errors were mechanical, and therefore simply inaccuracies of movement and due to the lack of adequate control, a study of the relations between the lengths of the movements and the errors was made. It showed that of two movements in the same direction, the shorter generally resulted in the larger number of errors. For example, right up, distance 11.2 inches, gave 9 errors, while right up, 29.7 inches, gave 4 errors; or again, left up, distance 36.1 inches, gave 8 errors, while left up, 10.4 inches, gave 12 errors. This principle applies only to mechanical errors. Woodworth (43) found that the accuracy of a movement is determined by "current control" governing the finer adjustments rather than by initial impulse. It is evident that the length of a short movement prevents the exercise of "current control," while in a long movement its operation is unhampered.

The marked correlation between the direction of movements and inhibitions (check on rapidity) suggested a similar comparison between direction of movements and errors (lack of accuracy) and dropped cards or lack of control. For this purpose the *absolute* number of errors and dropped cards as recorded in table 5 are arranged in a reduced form in table 9 according as they occurred with right or left movements and involving substitution or non-substitution of suit.

The number of movements to the right and left are equal, but those to the right sustain a handicap in that 25 per cent more of them distribute substitution cards. Despite this handicap the rightward movements show 45.9 per cent of the errors and 42.9 per cent of the dropped cards, while the leftward movements made 54.1 per cent errors and 57.1 per cent of the dropped cards.

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(No account is taken here of vertical movements.) While these differences are not marked they all point to a handicap in the *leftward movement of the right arm.* Or again if the hold-ups, "occurring during distribution" and in "memory" be tabulated according to *leftward* and *rightward* movements, there being 23 of each, and irrespective of feeling tone, it shows that 64.7 per cent hold-ups "during distribution" and 62.5 per cent for "memory" occurred with leftward movements, and that 35.5 per cent of hold-ups in "distribution" and 37.5 per cent for "memory" occurred with the rightward.

Our search for causal relations between feelings and the integration of movements into skilful patterns has shown that inhibitions and errors of whatever sort correlate with both feelings

TABLE 9

Showing relation between errors, dropped cards and direction of movement

DIRECTION OF MOVEMENT	NUMBER OF MOVEMENT,		ERRORS				DROPPED CARDS			
	Accompanied by substitution	Unaccompanied by substitution	Accompanied by substitution	Unaccompanied by substitution	Total	Per cent	Accompanied by substitution	Unaccompanied by substitution	Total	Per cent
Right.....	12	11	82	69	151	45.9	106	82	188	42.9
Left.....	9	14	79	99	178	54.1	99	151	250	57.1

and with direction of movements; and it further appears that, while feeling qualities may hinder or facilitate integration—at least of the grosser movements, the fundamental factor is the direction of movements.

The relative values of feelings of movements and of direction of movements in the acquisition of skill are considered as far as the facts will permit in the general summary.

D. General summary and conclusion

1. The feeling tone aroused by the application of stimulus to the sense organs may check or stimulate to some extent the operation of a skilful mechanism *already formed,* and it appears that

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U stimulus is more effective in this respect. There is but slight evidence that U stimulus affects in any way the growth of motor skill.

2. No statement can be made here concerning the effect of tense and relaxed feeling qualities on the integrating processes, although tense feelings were present more or less throughout the practice, but in many instances gradually faded with achievement.

3. Movements regardless of direction and of extent, but bearing cards for substitution, were more likely to be considered unpleasant than those bearing "resident" cards. The feeling excited by the cumulative effect of two successive substitutions became attached to the kinaesthesis of the movement for which the substitution was made and it was ascribed by the subject to the movement itself. It even affected in some instances the normal part of the distribution (club to club boxes and heart to heart boxes). There seems to be no doubt but that the feelings thus excited delayed the integration of the movements. How much of the delay was due to the feelings per se and how much to inhibitions caused by the clash between old and new associations could not be determined.

4. The feeling qualities aroused by the apparent conflict of the kinaesthesis, conditioned by critical movements, interfered with the rate of learning. Here as in "3," the question as to how much of the handicap was due to the conflict of kinaesthesis, and how much to feelings excited thereby could not, under the conditions of the experiment, be evaluated.

5. The U and P feelings excited by the moving arm in attempting skilful movements exerted a decided influence on the processes of integration. This conclusion is reached from two different sources of evidence: (1) The U feelings aroused by substitution of suit checked the learning and thus proved that feelings growing out of the learning process affected it. (2) The decided inverse correlation between P feelings and inhibitions and the positive correlation between U feelings (aroused by movements being integrated) and inhibitions.

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6. Perhaps even stronger elements than feelings for facilitating and checking the factors involved in the growth of skill are to be found, (1) In the direction of movements and (2) In the time and space relations of movements to each other, e.g., balanced and rhythmic movements. The latter condition undoubtedly affects the readiness with which movements integrate into systems. The literature on the bilateral asymmetry of the hands is indeed voluminous, but I fail to find any investigation attempting to determine the respective spatial zones in which each may achieve its maximum skill in accuracy, speed and control. These zones are doubtless conditioned and limited in the individual by his original neural capacity for making and controlling a variety of movements. The present study showed that the right hand is considerably handicapped in acquiring and making skilful movements to the left of the median plane of the body, and that the left hand, for the left handed (only one in this study), is also disadvantaged, although not seriously, in the acquisition of skill to the right of the median plane. The right hand sustained one third more inhibitions, dropped one sixth more cards, and was nine tenths as accurate while operating in the field to the left of the median plane. The more accurate studies of movements thus far have been limited to horizontal and to back and forth movements; there is need for determining the planes in which the movements of the arm, pivoting on the shoulder as a center, attain maximum skill.

It may be strongly contended that the direction of the movement is prior and fundamental to feeling and that both integration and feeling are results of reactions and that as results they do not function in integrating processes. I would readily assent to this view but for the evidence advanced in "(1)" and "(2)" of "5" above and in addition to the fact that direction of a movement is *not the sole determiner of its feeling quality*. To direction should be added rate, extent, and relation to other movements, as ballistic rhythmic, and systemic. Seventeen per cent of the movements to the left ranked high as pleasant and were easily learned, while 26 per cent of the rightward movements ranked high as unpleasant and were difficult to learn.

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7. The higher rates of skill were achieved through the capacity (1) to graduate the service of attention to the changing stages of skill, (2) to control the body as a whole in the interest of the hands, (3) to coördinate the movements of both hands, and (4) to use the thumb and fingers dextrously.

8. The slight use and aid of visual imagery in learning the location of the boxes and in distributing the cards was doubtless due to the stacked order. This gradually developed a system of movements that supplanted the service of the eye. At the close of the practice a special period was devoted to distributing the cards unstacked. This reduced considerably the use of the automatic system of movements and in endeavoring to recall a box, its visual image would sometimes appear, particularly of those boxes on the periphery or adjacent thereto. This condition for the appearance of visual imagery is somewhat similar to the task of spelling rather long words backwards, as employed by Dr. Fernald (16) in the "Diagnosis of Mental Imagery," where the necessity for conscious control forced up imaginal processes.

9. The practical bearing of this study was found in the fact that it determined the more salient traits necessary to become a competent railway mail clerk and a distributing clerk in central post offices. This aspect of the study will be reported later, in connection with a partial analysis of the processes involved in mail distribution.

REFERENCES

(1) ANGIER, R. P.: Die schatzung von Bewegungsgrossen bei Vorderarmbeweg-ungen. Zeitschrift f. Psychol. d. Sinnes. Bd. 39, 1905, S. 429.

(2) BEARD, G. M.: Physiology of mind reading. Pop. Sci. Mo., 1877, x.

(3) BERGSTROM, J. A.: Experiments upon physiological memory by means of the interference of associations. Amer. Jour. Psych., 1893, v, 356–359.

(4) BILLINGS, M. L.: The duration of attention. Psych. Rev., 1914, xxi, 121–31.

(5) BINET, A.: Qu'est-Ce Qu'ene Emotion. L'Annee Psychologique. 1911, xvii, 23–28.

(6) BRYAN, W. L., Development of voluntary Motor Ability, Amer. Sowr. psychol. 1892, vol. v, 125-204.

(7) BRYAN, W. L., AND HARTER, N.: Studies in the physiology and psychology of the telegraphic language. Psych. Rev., 1897, iv, 27–53.

Digitized by Google

(8) Book, W. F.: The psychology of skill with special reference to its acquisition in typewriting. Univ. Mont. Pub. in Psychology, 1908, Bulletin 53.
(9) Cleveland, A. A.: The psychology of chess and learning to play it. Amer. Jour. Psychol., xviii, 1907, 269–308.
(10) Coover, J. E., and Angell, F.: General practice effect of special exercise. Amer. Jour. Psychol., 1907, xviii, 328–340.
(11) Culler, A. J.: Interference and adaptability. Arch. of Psychol., 1912, xxi, no. 2.
(12) Diehl, August: Ueber die Eigenschaften der Schrift bei Gesunden. Kraeplin's Psychol. Arbeiten, 1901, Bd. III, 1–61.
(13) Downey, J. E.: Automatic phenomena of muscle reading. Jour. Phil., Psychol. & Sci. Meth., 1908, v, 650–658.
(14) Ebbinghaus: Grundzunge der Psychologie. 1911, Erster Bd. Dritter Auf. I, 538–566.
(15) Féré, C.: Sensation et Mouvement. 1887.
(16) Fernald, M. R.: Diagnosis of mental imagery. Psychol. Rev. Monog. Suppl. 1912, xlv, no. 1.
(17) Harvey, Nathan A.: The feelings of man. 1914, chap. ix.
(18) Hollingsworth, H. L.: Characteristic differences between recall and recognition. Amer. Jour. Psychol., 1913, xxiv, 532–544.
(19) Hollingsworth, H. L.: The inaccuracy of movement. Arch. of Psychol., 1909, ii, no. 13.
(20) Jastrow, J.: Science, 1886, viii, 237–241.
(21) Jastrow, J.: Studies from University of Wisconsin. Amer. Jour. Psychol., 1892, iv, 398–407.
(22) Johnston, C. H.: The feeling problem in recent psychological controversies. Psychol. Bul., 1908, v, 65f.
(23) Johnston, C. H.: The present state of the psychology of feeling. Psychol. Bul. 1905, ii, 161f.
(24) Judd, C. H.: Psychology, 1907, chap. vii.
(25) Kline, L. W., and Owens, W. A.: Transference and interference. Psychol. Rev., 1913, xx, no. 3.
(26) Kline, L. W., and Kellogg, Chester E.: Cards as psychological apparatus. Science, 1914, xxxix, no. 1009.
(27) McDougall, W.: Physiological psychology. P. 114.
(28) Mosso, A.: Fear. 1896, trans. by Lough and Kiesow.
(29) Müller und Schumann: Ueber die Psychologischen Grundlagen der Vergleichung gehobener Gewichte. Pflug. Arch., 1889, Bd. 45.
(30) Münsterberg, Hugo: Psychology and life. Pp. 50–51.
(31) Münsterberg, Hugo: Psychology and industrial efficiency. 1913.
(32) Münsterberg, Hugo: Beiträge zur Experimentellen Psychologie. Heft. 4, Gedachtnisstudien, 1892, 75f.
(33) Pillsbury, W. B.: The place of movement in consciousness. Psychol. Rev., 1911, xviii, 83–99.
(34) Royce, Josiah: Outlines of psychology. 1903.
(35) Rose, H.: Der Einflusse der Unlustgefuhle auf den Motrorischen Effekt der Willenshandlungen. Arch. f. d. Gesamte Psychologie, Bd. 28, 1913, S. 94–182.

Digitized by Google

(36) SMITH, T.: Motor Memory. Amer. Jour. Psychol., 1896, vii, 453–490.
(37) SWIFT, E. J.: Psychology and physiology of learning. Amer. Jour. Psychol. 1903, xiv, 201–251.
(38) STETSON, R. H.: Theory of rhythm, and discrete succession. Psychol. Rev., 1905, xii, 250–270.
(39) STÖRRING, G.: Experimentelle Beiträge zur Lehre vom Gefuhl. Arch. f. d. Gesamte Psychol., 1906, Bd. vi, 316–356.
(40) TITCHENER, E. B.: A text-book of psychology, 1912.
(41) WATSON, J. B.: Psychology from the standpoint of a behaviorist. 1919.
(42) WASHBURN, M. F.: Speed of motor learning. Amer. Jour. of Psychol., 1913, xxiv, 580–583.
(43) WOODWORTH, R. S.: Accuracy of voluntary movement. Psychol. Rev. Monog. Suppl., 1889, iii, no. 3.
(44) WOODWORTH, R. S.: Le Mouvement. Paris, 1903.
(45) ZIEHEN, T.: Leitfaden der Physiologischen Psychologie. 1906.

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SOME TRANSFER FACTORS IN MAZE LEARNING BY THE WHITE RAT

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The question as to the effect of one habit formed upon the acquisition of a new habit is one that has had a goodly share of attention from the psychologists in so far as it affects human learning. When recognized as a part of the field of "transfer of training" its experimental treatments are recognized as legion. Practice on this, that, or another habit has been found to have just so much—or so little—helpful or hindering effect upon the development of a second habit. Experimental data have been mustered to show that: training in discriminating sound intensities seems to help in discriminating shades of gray; memorizing four tones favorably affects the memorizing of geometrical figures or remembering extent of arm movement; learning of nonsense syllables helps in the later learning of Italian words or in the learning of numbers; practice in inscribing the letter X in squares helps later in the inscribing of various geometrical figures, etc. And whether the transfer effects are as great as some think or not, various suggestions have been offered as to the factors at work. Some say that it is a matter of identical elements in the two kinds of content; some ascribe more virtue to the carrying over of incidental attitudes, of ideals, etc. But in the vast majority of experimental studies these conclusions, in so far as they become detailed and explicit, have been more or less largely inferential. What frequently seems needed is definite, clear isolation of just what was transferred.

Now, one of the important functions of animal psychology is the yielding of fruitful points of view for human psychology. The workings of the animal's mind being complicated by usually

[1] The experimental work was done in the Oberlin College laboratory.

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less delicate innate capacities and by less intricately involved effects of experience than is that of the genus *Homo*, the simpler forms which problems in animal psychology assume may often lead to results more unequivocal and more illuminating in specific ways. Unhappily the study of the effect of the formation of one habit upon the acquisition of a new habit with animal subjects has not been far pressed. Only a few reports have been made along this line.

R. M. Yerkes[2] and W. S. Hunter[3] have obtained opposite results as to the value of previous maze practice for the learning of a new maze by the dancing mouse and by the pigeon, respectively. H. H. Wylie[4] has reported the degree of transfer effect between an earlier learning of a reaction to a given type of stimulus and the later learning of a reaction to a different type of stimulus in the same general situation. W. S. Hunter and J. U. Yarbrough[5] found for the white rat a forward-acting interference between a first auditory-motor habit in a discrimination box and a second auditory-motor habit in the same box; and B. D. Pearce[6] got similar results with visual-motor habits. L. W. Webb[7] with the white rat found positive transfer between an original maze habit and each of five other maze habits. H. A. Ruger[8] found no transfer of "semicircular canal practice" in the maze by rats. R. T. Wiltbank[9] has made a study of the learning of rats in pairs and in series of mazes, especially as to quantitative relations. In his theoretical analysis of transfer from maze to

[2] The Dancing Mouse, New York, 1907, pp. 261 ff.

[3] Some labyrinth habits of the domestic pigeon, Journal of Animal Behavior, i, 1911, 278–304.

[4] An experimental study of transfer of response in the white rat, Behavior Monographs, iii, 1919, no. 16.

[5] Interference of auditory habits in the white rat, Journal of Animal Behavior, vii, 1917, 49–65.

[6] Note on interference of visual habits in the white rat, Journal of Animal Behavior, vii, 169–177.

[7] Transfer of training and retroaction, Psychological Monographs, xxiv, 1917, no. 104.

[8] Some experiments in the transfer of habits in the white rat (abstract), Psychological Bulletin, xv, 1918, 42.

[9] Transfer of training in white rats upon various series of mazes, Behavior Monographs, iv, 1919, no. 17.

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maze he suggests as elements: the recession of the instinct of timidity, the association between maze situation and food, and practice in error elimination. These may well be given experimental determination by laboratory applications of the "methods of agreement and difference." This monograph appeared after the present experiments were completed. References to Wiltbank's monograph will be made below.

The attention of the writer turned to the study of the possibility of the experimental isolation of factors that might be transferred. The material devised and used in the Oberlin laboratory seemed unusually well adapted to such an investigation: being in a multiple unit system capable of indefinite rearrangement of maze patterns.[10]

I. FIRST SERIES

The work was undertaken by some members of the writer's class in animal psychology, the students performing much of the routine of the work but with the constant presence and oversight of the instructor. Nineteen rats were used as subjects,

TABLE 1

	GROUP I	GROUP II	GROUP III	GROUP IV
Litter 5 weeks old.....	2	2	1	1
Litter 7 weeks old.....	2	1	2	1
Litter 11 weeks old.....	1	2	2	2

taken from three different litters, and so re-grouped as to have some from each litter in each of the four groups to be used (see table 1). The four groups were given distinct kinds of "training" or preliminary work, and all were given the same "test" problem, thus rendering possible some judgment as to the relative values of the different sorts of preliminary practice.

In detail: The preliminary training for group I consisted only in taking them from the nest at regular feeding time, placing them

[10] For description, see R. H. Stetson and J. F. Dashiell, A multiple unit system of maze construction, Psychological Bulletin, xvi, 223–230.

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upon a table top upon which had been set the food box, and letting them learn to enter the box by pushing open the wire mesh swinging door. This was a simple matter and learned at one trial. The process was repeated for a total of seven trials. No maze of any sort was used. A similar introduction to food box

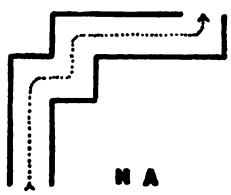


FIGURE 1.

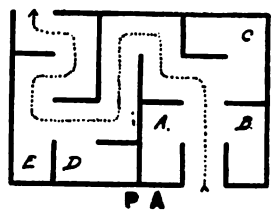
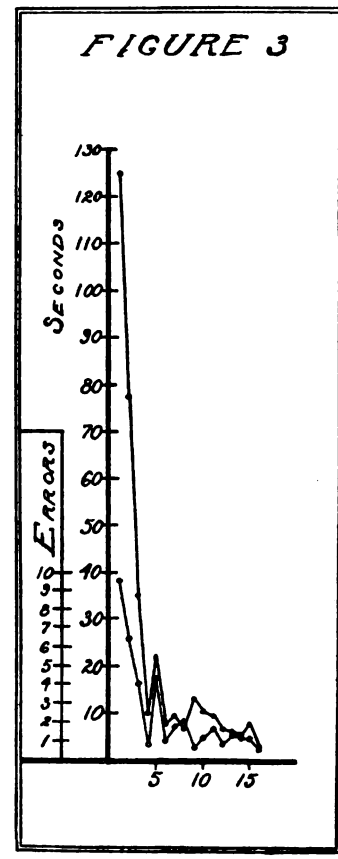


FIGURE 2.



and door was given all the other groups just preceding the preliminary training to be next described.

Group II was made only to pass through the single passage NA (no blind alley), figure 1, before entering the food box. This passage way was constructed of the same materials (cork floor, iron sides, glass cover) as used in the mazes of the training series

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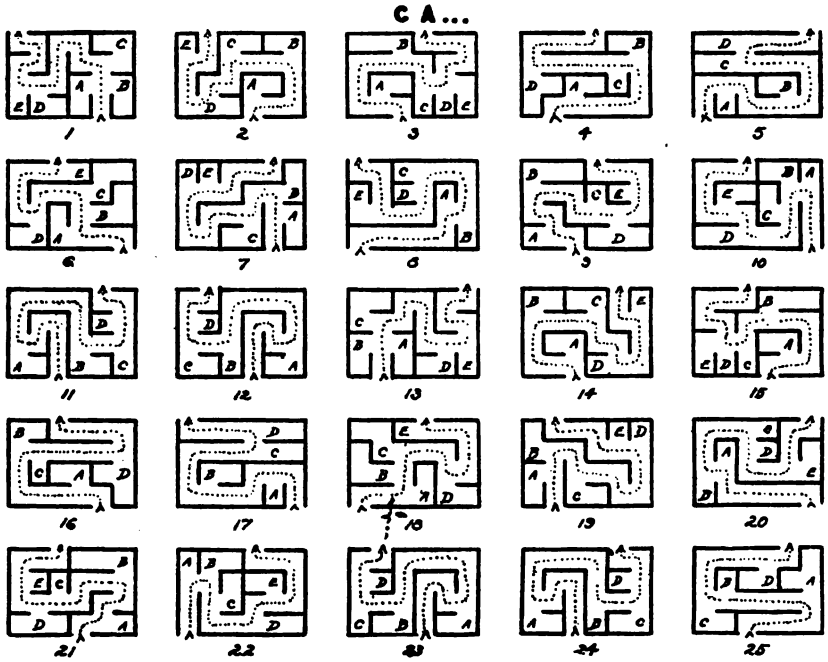
of groups III and IV and in the mazes of the test series for all four groups. This run was repeated daily for a total of ten trials. Time was not taken for these runs.

Group III was taught a definite maze problem in reaching the food. The plan used was the PA maze (permanent blind alleys) in figure 2, and the course of learning in this case is given graphically for the whole group in figure 3, shown only to include the

FIGURE 2.



16th trial as different individual rats had learned the problem and had been eliminated from trials later than this. The curve drawn is seen to be of the usual type. Only one of the five rats had not learned the problem (three errorless trials in succession) by the twenty-fifth or last trial, and it had run without error on the twenty-first and twenty-fourth trials. In order to keep the group more or less together this rat was treated as having learned.

Group IV was given the rather unusual task of running twenty-five different mazes on twenty-five successive days, one trial per

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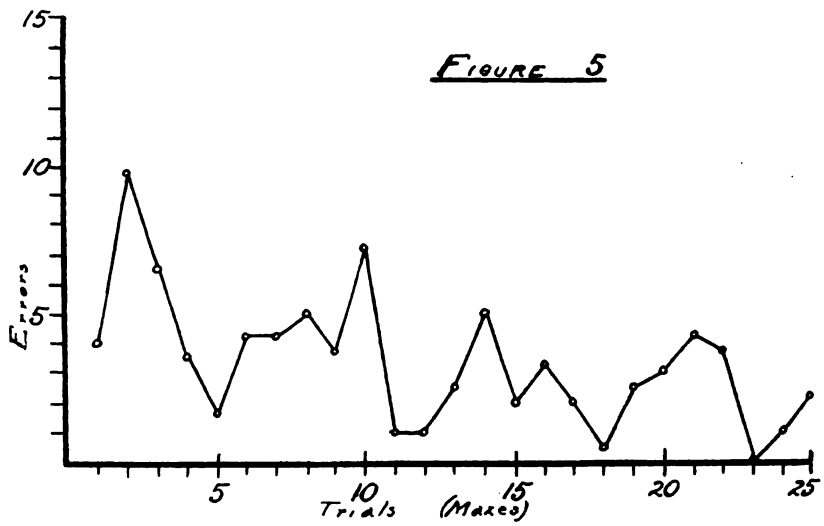
day. Thus the CA 1 maze of figure 4, identical with the PA maze of figure 2, was run the first day, CA 2 the second day, CA 3 the third, and so on for the twenty-five daily trials. The changes made in the maze patterns between each two successive trials were not great; and certain constant features were preserved throughout: the outside dimensions remained identical, the entrance door was always on the same side, and the exit door on the same side (opposite to the side on which the entrance door was placed).



The results of the twenty-five trials are presented graphically in figure 5. A slight, very general, downward slope seems to indicate improvement, but interpretation is difficult because of the extreme irregularity of the curve, and especially because what is indicated is not only the improvement in the subjects' reactions but also (perhaps mainly) the differences in difficulty to be found between the various mazes used. A somewhat more definite way of measuring whatever improvement there was in the subjects' reactions is given in table 2. Here are set down all the cases of repetition of the same error in the same trial. In the running of maze CA 3, for example, the blind alley A was

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entered three times by one of the four animals of the group, two times by another; alley B was entered four times by one rat, twice by each of two rats; alley C was taken three times by one rat; alley E was taken twice each by two rats. (Of course, the alley names, A, B, C, D, E, do not refer to the same alleys in successive mazes—the method of tabulation is used for convenience.) Apparently, the animals were gradually learning not to re-enter a *cul-de-sac* previously entered on the same run.

Comparing the different methods of training for the four groups, we see that group I was given no acquaintance with any feature of the maze to be used later for testing. Group II was given acquaintance with the sorts of materials of which the maze was to be constructed, but was given no *culs-de-sac* to learn to

TABLE 2

NAMES OF BLIND ALLEYS	NAMES OF MAZES (CA . . .)																							
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
A	3	335	32	32		4	2	3	2	243			2	3		2				2		3			2
B		426	422			4	2	3	232	222				2		22					225	3			
C		35	3			5		4		222			2	3											
D					2		3																		
E			22				2						2	3											

eliminate and hence no true maze situation. Group III was given a complete maze problem of the usual type. Group IV was given mazes but was not allowed to mechanize the true path in any case, having always new blind alleys to chance into. If this group were to show after effects of the twenty-five trials, they would be due obviously not to the learning of specific turns and twists as in the case of group III, but to the development of more general traits.

The testing for transfer effects was given in the form of the maze problem Q shown in figure 6. Unfortunately, the results were invalidated after the eighth trial by an accidental factor. The records after this trial were very irregular, the behavior of the subjects on a following day being almost unpredictable. This was not only quite unlike the results of other investigators but

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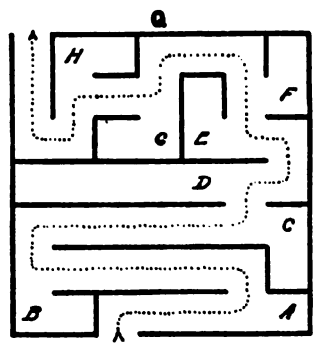
also unlike all the results which the writer and his students had previously gotten in work with these animals and this material. It was soon after discovered that wild gray rats had lately found their way into the building where the experiments were being conducted and had even left their trail in the mazes.

The exact form and degree of disturbance produced in the white rats by the presence of the traces of the gray pests is a thing yet to be made out in precise terms, but that there is a general disturbance produced seems unquestionable from the writer's observations. It may be mentioned incidentally that

FIGURE 6.



before the gray rats had been completely disposed of a few of them had forced their way into nests of the white rats, wounding or killing some of the inmates.

The average rates of learning of the test maze Q for the eight trials mentioned is given for the various groups in figure 7. The hump in the curve for group III on the second trial is due to the irregular performance of two of the five subjects of this group, and does not seem significant inasmuch as (a) the three other rats showed decrease in time in the second over the first trial, and (b) four of the five subjects showed decrease in errors on the second trial.

Comparing records for groups I and II, little difference is brought to light except a slight superiority for group II in both the matter of time and the matter of errors after the fourth trial.

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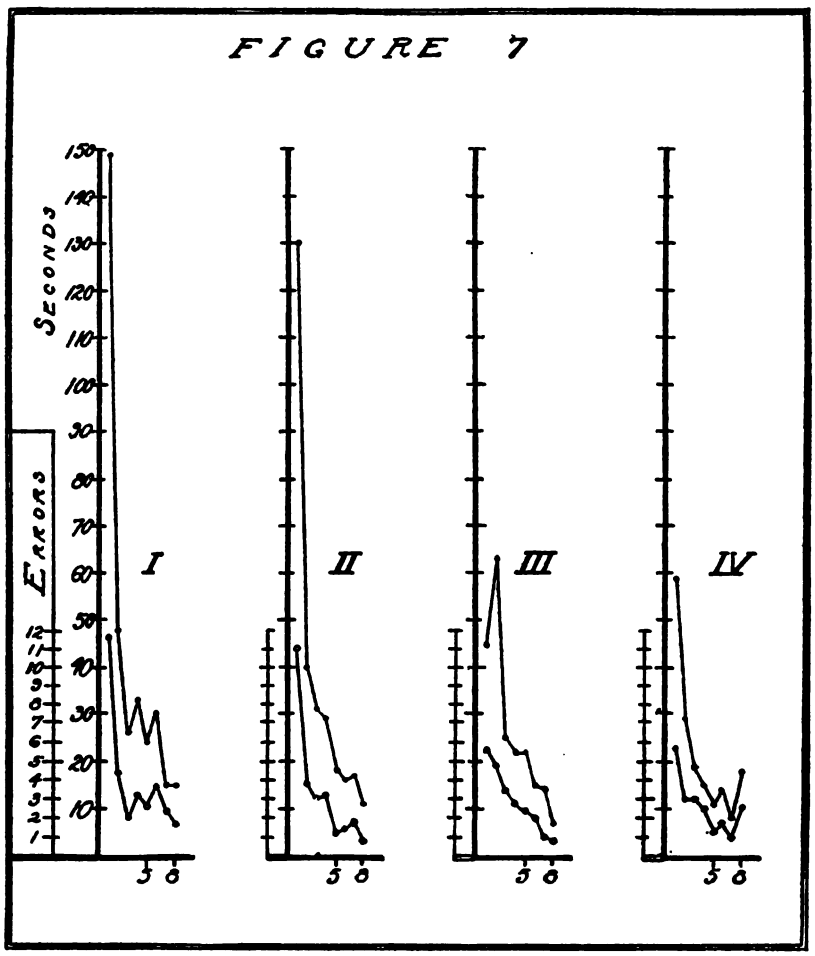
It will be recalled that group II had had acquaintance with a passage way built of the material used later in the maze Q while group I had had no acquaintance with it; but the difference in scores is comparatively slight. Again, in comparing the records

FIGURE 7



for groups III and IV we see the rate of improvement for III to be slightly faster and slightly more regular (barring the time record for III's second trial); but that these differences are due to group III's having learned one single maze completely and group IV's having run a new maze daily we cannot assert.

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The one outstanding difference in the records is that of the initial trials of groups I and II on the one hand and of groups III and IV on the other. Groups I and II show for the first trial, in time, 149.8 and 130 seconds, respectively, in errors, 11.6 and 11, respectively. Groups III and IV show for the same, 49.2 and 59.5 seconds, respectively, and 5.6 and 5.7 errors, respectively. A note made just after the first test trial reads: "Conspicuous feature of rats of III and IV on first trial in Q = absence of hesitancy in general behavior." In the further course of learning, however, no marked differences emerge, the averages of groups III and IV being only slightly better than those for I and II. Now the one difference between the methods of preliminary training for groups I and II taken together and for groups III and IV taken together is in the non-presentation and the presentation, respectively, of blind alleys or *culs-de-sac*. That an animal is made previously acquainted with the physical materials of which his test maze is to be constructed, seems, then, to be decidedly less important than that he be given acquaintance with some blind alleys; and furthermore, experience in gradually eliminating blind alleys seems to be decidedly less important than mere acquaintance with the alleys. (Compare group III with group IV.) The latter point seems further borne out by the observation that if training in actual elimination of blind alleys were highly important it would surely show itself in an accelerated improvement after the first trial and throughout the learning process for group III—which is only to a slight degree the case.

A conclusion to be drawn from this part of our study would be: When training of white rats in one maze is found to transfer or carry over to the learning of another maze, the principal factor that is transferred is adjustment to maze-situation-in-general.[11] Possible secondary factors are adjustment to physical materials and adjustment in the form of elimination of constant errors in successive runs in one maze. A practical corollary that follows is that for the learning of a maze *de novo* no rat should be used that has previously been run in any kind of maze whatever.

[11] Wiltbank (*op. cit.*, p. 35) would explain this positive transfer in early trials in terms of the recession of the instinct of timidity. This would seem more relevant to the elimination of surplus time than of errors.

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The transference of the adjustment to maze-situation-in-general affects principally the initial trials in the new maze. Here we seem to have a case of preliminary adjustment to a problem-situation of a general type analogous to the preliminary adjustment found to play a large part in human learning. Apparently, a rat in learning a maze under the conditions of the usual type of experiment has for a first problem the getting used to the presence of the *culs-de-sac*, and the very steep initial descent of his learning curve is due in part to this fact.[12]

An incidental part of the experimentation above reported gave evidence of another factor of transfer. As described, the subjects of group IV were in their training series run with each single daily trial in a new maze. The series of mazes used is shown in figure 4. It was stated above that (a) the outside dimensions of all mazes remained constant, (b) the entrance door was always on the same side, and (c) the exit door and food box was always on the same side, and opposite the entrance. As to results, it was indicated above that the animals of this group seemed gradually to learn not to repeat an error during the one run in each maze (table 2). Now, furthermore, after the first few trials the experimenter observed, or thought he observed, that the animals were developing a sense of direction for the food box; i.e., that whenever confronted with a choice of paths they tended to turn toward it rather than away from it. By this is meant a turning toward the food box side of the maze, which was constant, and not necessarily toward the door of the food box as it occurred in that particular maze. (That they could not have learned the location of the food box in terms of true path or paths to be taken is obvious from the conditions of the problem—ever-changing *culs-de-sac*.) To get an exact proof or disproof of this rough observation the writer constructed a table as follows (table 3). Each maze pattern was inspected and the blind alleys classified under three heads: (a) those which when entered involved a turn by the subject toward the food box side of the maze; (b) those which involved a turn away from that side and toward the

[12] Wiltbank (*op. cit.*, pp. 36–37) speaks similarly of "error elimination activity" as transferable.

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TABLE 3

NAMES OF MAZES (CA . . .)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	
Alleys leading *toward* direction of food box	C 3	A 12 B 13 C 8 E 5	A 5 B 9	A 5 B 5	B 2	A 5 B 4 C 7	B 4 D 6 E 2	A 4 E 3	B 8 C 0	A 10 B 7 C 7 E 2			C 3	A 6 B 4 C 5 E 5	A 2 B 4	A 5 B 5	B 4	A 0 B 1 C 1	B 2 D 4 E 3	A 4 E 3	B 10 C 3	A 5 B 5 C 1 E 0			A 5 B 3	Total alleys presented 50 Total times entered.. 229 Average..... 4.59
Alleys leading *away from* direction of food box	A 4 B 4 D 3 E 2		C 5 D 2 E 5	C 3 D 1	A 1	D 1	A 3 C 2	B 3	D 3		A 2 B 0 C 2	A 2 B 0 C 2	A 3 B 0 D 2 E 2		C 0 D 0 E 2	C 0 D 3	A 1	D 0	A 1 C 0	B 0	D 1		A 0 B 0 C 0	A 0 B 2 C 2	C 1 D 0	Total alleys presented 44 Total times entered.. 70 Average..... 1.59

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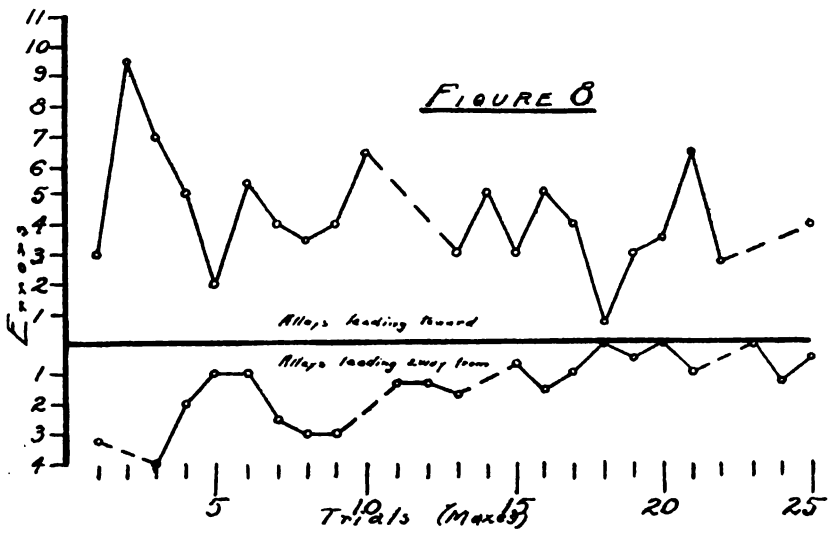
entrance side; and (c) those involving turns toward the other two sides and hence neither toward nor away from the food box. The third (c) type were discarded as irrelevant or at least equivocal for this problem. The blind alleys for the first (a) and second (b) types were listed in two different columns for each successive maze, and count was then taken for the number of times each had been entered by all the rats of the group. Averaged totals show that a blind alley turned in the food box direction was entered on an average of 4.58 times by the four rats, while one turned away



from food box was entered on an average of 1.59 times. This is a substantiation of the rough observation first made: that the animals came to show a fairly definite tendency in the food box direction that was transferred from maze to maze. Evidence that this tendency was gradually developed in successive runs, being absent at first and strong toward the end, is presented graphically in figure 8. It shows the total number of entrances into mazes of the (a) type and again of the (b) type for the whole group, each divided by the number of mazes of each type offered. It is based upon table 3. In considering this figure the reader should keep in mind the fact that all errors tended to be reduced somewhat,

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as shown in figure 5; and also that, as before remarked, changes in vertical distances on the graph indicate differences in the mazes and alleys used in different trials as well as changes in subjects' ability.

This matter of orientation to food direction is in line with general observations on the behavior of very many (perhaps all) higher animals when seeking their homes. The gray rat, for instance, does not always follow the same path back to his hole in the wall; and especially when partly blocked in his efforts to return by obstacles or enemies in his path, will adopt circuitous routes about the side walls or, in rare cases, across the open floor, sometimes even progressing by stages from cover to cover, always in the general direction of his hole.

The matter may be summarized: In a series of changing mazes with food box in one constant direction from the entrance, a blind alley turning toward the general direction of the food box is more likely to be entered than one turning away from this general direction, in about the ratio of 3 to 1. This developing tendency forms a factor in the transfer of maze training.

Two criticisms may have suggested themselves to the reader:

1. Was not the developing orientation in the food box direction due to olfactory sensations? Unhappily, no experimental check was employed on this question (the fuller possibilities of this method of training group IV in the changing mazes were, indeed, not in the experimenter's mind until the results came to be worked over several weeks after). Internal evidence of a kind is, however, to be found. Inspection of the maze designs used (fig. 4) in connection with the frequency table of most of the errors made (table 3) will bring to light many cases of turns into blind alleys at points where olfactory stimuli would surely have led otherwise. See, for instance, these mazes and alleys: 3, C and E; 6, B; 7, D and E; 8, B; 9, D; 11, A and C; 12, A and C; 13, C; 14, E; 16, D; 19, D; 20, E.

2. Was not the orientation to the food box really an orientation to cardinal directions (to a point of the compass)? This matter of orientation to absolute direction, irrespective of any particular points or cues, seems to the writer a highly dubious idea. That

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an animal could be sensitive to direction-in-general regardless of what his auditory, visual, olfactory, kinesthetic, and other stimuli are, is a proposition almost defying interpretation in psychological terms. The matter may be given point by repeating an assertion made by a former colleague of the writer: "If I were to find an animal orienting himself in a certain absolute direction I should at once want to discover what the particular cue is that is guiding him." That numerous cues may so function is conceivable and an unravelling of the efficient cues and of their respective rôles would make material sufficient for a complete series of experimental studies by itself. Any slight sloping of the maze floor, any shadows cast, the presence of the experimenter in a constant position, the presence of a post or piece of furniture, a constant difference of temperature on different sides of the room, a constant direction from the nests where other rats are feeding and making slight noises—these and other factors are all conceivable as effective here. For the present study, however, the writer must be content with having shown that a general orientation in what was the food box direction, whatever the cues used, became gradually established through a series of changing mazes having this direction constant.

II. SECOND SERIES

The foregoing experiments were carried on in the winter of 1918–19. The following spring and summer a complete repetition of them was made for two reasons. In the first place, it was felt that on account of the limited number of subjects used, the experiments could be called only preliminary; whereas a checking of results by a new set of animals used at another time would give them more definite value. In the second place, the ending of the curves for the experiments with the test maze Q at the 8th trial (fig. 7) is unsatisfactory because it leaves partially undetermined whatever the relation is between the previous practice on the other mazes and the later runs on this. Does previous maze practice affect only the initial attack upon a new maze or does it accelerate or retard the whole learning process? The

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answer has been suggested above but not definitely shown in terms of data. In this series the general description of the procedure of the experiments can be much abbreviated.

The ages and grouping of the rats are shown in table 4.

All groups were first given opportunity to learn to push open the door of the food box when placed on a table. Group I had this practice only before its formal maze trials. It was given this seven times.

TABLE 4

	GROUP I	GROUP II	GROUP III	GROUP IV
Litter 9 weeks old	1	1	2	2
Litter 11 weeks old	1	2	2	1
Litter 12 weeks old	2	2	1	2
Litter 14 weeks old	1		1	

TABLE 5

SUBJECTS	TRIALS									
	1	2	3	4	5	6	7	8	9	10
a	5	4	4	3	3	3	2	2	2	3
b	6	8	6	4	4	3	7	3	3	3
c	6	3	8	3	4	3	3	3	2	2
d	8	6	5	5	4	4	4	4	3	2
e	3	3	4	2	3	2	2	4	3	3
Averages..	5.6	4.8	5.4	3.4	3.6	3.0	3.6	3.2	2.6	2.6

Group II was given the run through the single passage NA (fig. 1) for ten trials. Time was this time taken. The group as a whole showed a fairly regular decrease of time as given in table 5.

Group III in twenty-five trials or less in maze PA performed almost exactly as did the group III of the earlier series, showing a learning of so similar a type that space need not be given to its scoring.

Group IV in twenty-five trials in the changing mazes CA gave results showing graphically in figure 9. In comparison with figure 5 we find here the same very general downward trend

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with extreme irregularities. It is to be observed that the particular "ups and downs" do not correspond well with those of figure 5 (barring those for mazes numbered 4–5–6 and 9–10–11–12), and they differ markedly, indeed, in the data for the individual rats. Table 6 shows for this group, as table 2 did for

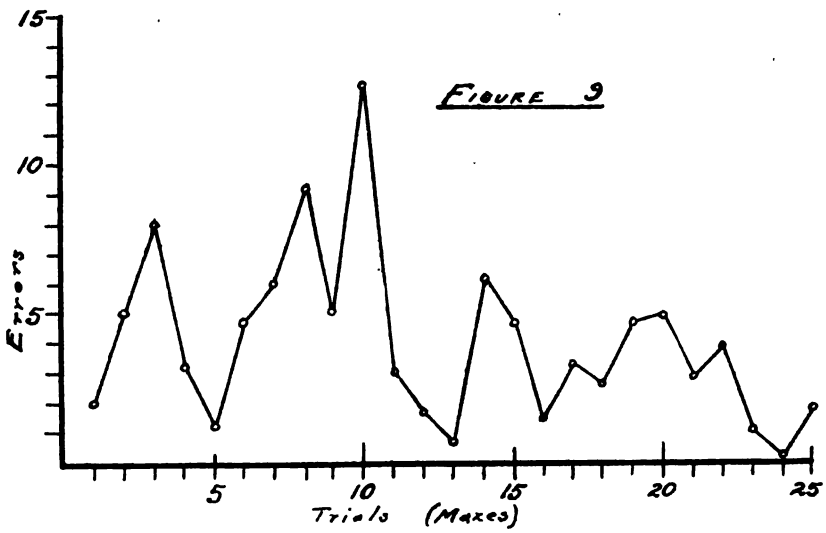

TABLE 6

NAMES OF BLIND ALLEYS	NAMES OF MAZES (CA . . .)																							
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
A	2	23	2422	2		23		42	2	2227	22			232	2		2			2	2				2
B		23	2242	2		2	26	2	322	328				3	2		32		3		22	2			
C			2			23	2	534		36				2				2		3					
D				2			2	4		2									2	2					
E		22	4							3					2										

group IV of the first series, a positive improvement in the form of the development of a tendency not to re-enter a blind alley previously entered on the same run. That this decrease in number of re-entrances is not due to the objective character of the mazes used is suggested by the fact that, aside from this similarity of general trend, the two tables are not alike. Outside of

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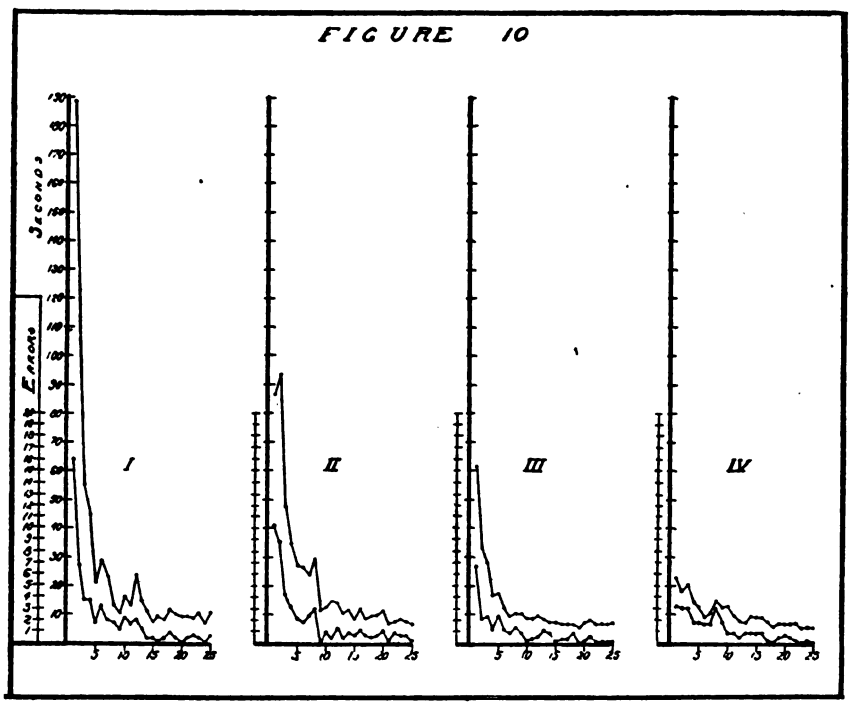
mazes 9 and 10, the same alleys (in the same mazes) do not show anything like a similar number of re-entrances.

Remembering the different sorts of preliminary training or "practice series," we may now consider the formal experiments or "test series." All four groups of rats were run for this crucial experiment in the maze Q (fig. 6). In the first section of the present paper the results of the test series with the first supply of

FIGURE 10



rats used were given only in part, the learning of the rats being disturbed after the 8th trial by an extraneous factor unforeseen (fig. 7). As amplification of that record the new series with the second set of rats has great value. In graphic form the record appears in figure 10. Here we have gradations between all the four curves more definitely shown than in figure 7. Group I in this case shows substantial inferiority to group II in its initial attack upon Q; though in later trials the differences are insignifi-

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TABLE 7

NAMES OF MAZES (CA . . .)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	
Alleys leading *toward* direction of food box	C 1	A 6 B 8 C 0 E 6	A 11 B 10	A 3 B 6	B 1	A 5 B 3 C 7	B 11 D 6 E 2	A 7 E 5	B 8 C 1	A 14 B 14 C 10 E 5			C 0	A 8 B 5 C 5 E 3	A 5 B 5	A 4 B 3	B 6	A 2 B 4 C 4	B 7 D 4 E 4	A 3 E 4	B 6 C 2	A 3 B 5 C 3 E 3			A 5 B 3	Total alleys presented 50 Total times entered.. 256 Average..... 5.12
Alleys leading *away from* direction of food box	A 4 B 4 D 0 E 1		C 3 D 0 E 7	C 1 D 1	A 0	D 2	A 3 C 2	B 2	D 2		A 4 B 2 C 0	A 5 B 1 C 2	A 0 B 1 D 0 E 2		C 1 D 1 E 5	C 0 D 0	A 3	D 0	A 3 C 1	B 1	D 0		A 3 B 0 C 2	A 0 B 0 C 0	C 0 D 0	Total alleys presented 44 Total times entered.. 69 Average..... 1.57

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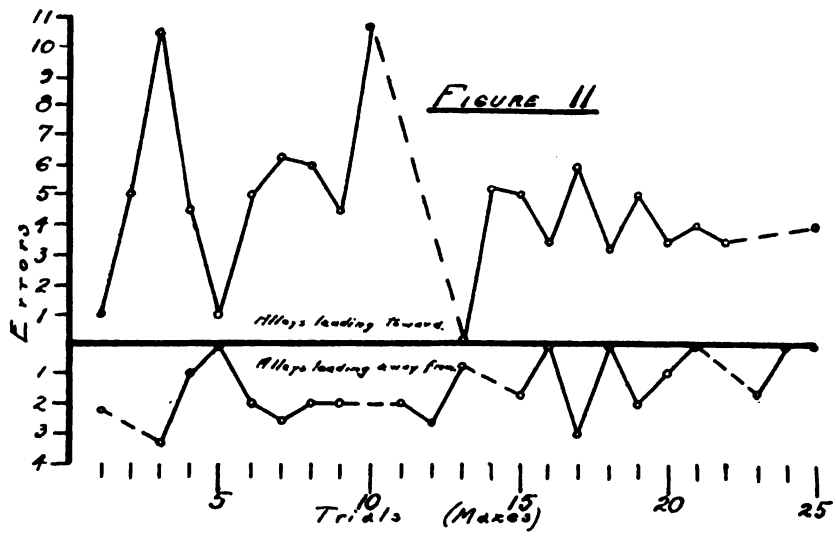
cant. Does this less amount of time and errors for group II on the first run measure the effect of adjustment to maze materials as a transfer factor? (*Supra*, p. 337.) The difference between the records in Q of groups III and IV seemed negligible in the first series of experiments; but in figure 10 we see them clearly shown in the initial attack, a comparison of later runs being inconclusive. Finally, the difference between groups I and II taken together and groups III and IV taken together is again found to hold; suggesting, as was stated above, that the mere presence of blind alleys in the environment of the rat has signifi-



cance and familiarity with such is transferable. Finally, the records for the four groups differ significantly only in the earlier readings, showing that the transfer effects from practice in previous mazes affects principally the first few trials in a new maze. Thus, the second series of experiments serve not only to corroborate the results of the first series in the matter of differences between groups I and II and groups III and IV, but also to render more definite the differences between group I and group II and again between group III and group IV.

The incidental runs of group IV in the series of mazes CA . . . again are interesting. It will be recalled that the con-

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stant features of all the CA mazes were (a) the outside dimensions, (b) the side upon which entrance was placed, and (c) the side upon which exit was placed; and that the animals ran in each maze only once. Table 7 is given for comparison with table 3, above. Averaged totals of entrances into alleys turned toward the food box and of entrances into alleys turned away from it show that the former alleys were entered on an average of 5.12 times, the latter on an average of 1.57. These figures bear out those above (4.58 and 1.59, respectively). Evidence of a rather gradual development of this tendency to enter alleys turned in the food direction and not to enter those turned clearly away from it is given in figure 11 (for comparison with figure 8, above).

SUMMARY

We have attempted to isolate some of the elements involved in the transfer of training from maze to maze by the employment of a variety of maze patterns. Four groups of white rats were given four different kinds of preliminary training, and all then tested in one maze. As to results, we seem to have isolated as factors in the transfer of maze learning ability: (1) adaptation to a general maze situation, i.e., presence of *culs-de-sac;* (2) adaptation to the materials out of which maze is built (a possible small transfer); (3) the habit of eliminating certain constant errors (a possible small transfer); (4) adjustment in the form of a tendency not to repeat an error in a single run; (5) definite orientation to food box direction.

A corollary of general import suggests itself. The literature on the transference of training in human subjects has seemed to work out three general types of factors transferable: (1) identities in content, (2) identities in procedure, (3) identities in aim or ideals. These three appear to the writer as classifiable into two groups: (A) the particular habits or more or less mechanical sensori-motor responses to particular elements of the situation, (1) above; and (B) the more general aspects of adaptation of attention, use of imagery, etc., given subjective interpretation either as to their formation or as to their operation, (2) above, as

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well as clear ideas and clear ideals, (3) above. Now, our work with animals here reported seems to indicate that not only particular elements in sensori-motor response to particular elements of situation but also general types of motor response to general types of situation are transferable—and not (we think any comparative psychologist will hold) to be interpreted as conscious adjustments or attitudes or ideals or other processes explainable in subjective terms. Can it be that the objective or sensori-motor aspect of learning has been inadequately understood, and that all the possible factors of transfer on this plane have not been fairly recognized? Or, to put the matter somewhat differently in certain well-known terms, is a generalized habit (i.e., a generalized motor habit) a possibility of training and therefore of transfer? And, finally, can this be relevant to human as well as to animal learning?

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THE DYNAMOGENIC INFLUENCE OF LIGHT ON TACTILE DISCRIMINATION[1]

H. M. JOHNSON

This paper reports a minor study made at the Nela Research Laboratory in 1916. It was incited by interest in the question of one sensory process tending to reinforce or to inhibit another sensory process belonging to a different mode.

A single commonplace illustration may suffice to fix the problem. It is often asserted that a smoker cannot tell in the dark whether his tobacco is burning or not. The assertion is somewhat too sweeping. However, a fairly large group of smokers casually interviewed by the writer reported unanimously that they do not enjoy smoking in the dark; and that they cannot experience some of the gustable and olfactable qualities of the smoke unless they can see it. Certainly such is the case with the writer, who smokes habitually and heavily.

HISTORICAL

The historical notes given below are necessarily incomplete. For some two years the present writer has not had access to literature on the topic, nor even to old historical memoranda prepared by himself, which are stored with his household effects. His present duties probably will not permit of further examination of the literature for some time to come. Interested readers are urged to find the original sources for themselves.

James[2] writes as follows:

> The whole neural organism, it will be remembered, is, physiologically considered, but a machine for converting stimuli into reactions; and the intellectual part of our life is knit up with but the middle or

[1] This paper from the Nela Research Laboratory, National Lamp Works of General Electric Company, Nela Park, Cleveland, Ohio.

[2] James, William: Principles of Psychology, ii, 372 ff. New York, Henry Holt & Co., 1890.

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'central' portion of the machine's operations. . . . Every impression which impinges on the incoming nerves produces some discharge down the outgoing ones, whether we be aware of it or not. . . . .

There are probably no exceptions to the diffusion of every impression through the *nerve-centers*. The *effect* of the wave through the centers, may, however, often be to interfere with processes, and to diminish tensions already existing there; and the outward consequences of such inhibitions may be the arrest of discharges from the inhibited regions and the checking of bodily activities already in process of occurrence. When this happens it probably is like the draining or siphoning of certain channels by currents flowing through others. When, in walking, we suddenly stand still because a sound, sight, smell or thought catches our attention, something like this occurs. But there are cases of arrest of peripheral activity which depend, not on central inhibition, but on stimulation of centers which discharge outgoing currents of an inhibitory sort. . . . .

In general, however, the stimulating effects of a sense-impression predominate over the inhibiting effects, so that we may roughly say . . . . . that the wave of discharge produces an activity in all parts of the body.

James here cites reports of Haller, Mosso, Féré, Tarchanoff and others as indicating that visual, olfactory and auditory stimuli of short duration produced changes in the circulation, respiration, secretion of the sudorific glands, and strength of contraction of voluntary muscles.

Perhaps the reference most nearly relevant to our experiment is to the work of Féré. I quote again from James:

Every sensorial stimulus not only sends a special discharge into certain particular muscles dependent on the special nature of the stimulus in question . . . . but it innervates the muscles generally. M. Féré has given very curious experimental proofs of this. The strength of contraction of the subject's hand was measured by a self-registering dynamometer. Ordinarily the maximum strength, under simple experimental conditions, remains the same from day to day. But if simultaneously with the contraction the subject received a sensorial impression, the contraction was sometimes weakened, but more often increased. This reinforcing effect has received the name *dynamogeny*. The dynamogenic value of simple *musical notes* seems to be

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proportional to their loudness and height. Where the notes are compounded into sad strains, the muscular strength diminishes. If the strains are gay, it is increased. The dynamogenic value of *colored lights* varies with the color.

In an hysterical subject, whose sensibility was higher than the normal, and "whose normal strength was expressed by 23, it became 24 when a blue light was thrown on the eyes, 28 for green, 30 for yellow, 35 for orange and 42 for red. Red is thus the most exciting color." The dynamogenic effects of gustatory and olfactory stimuli are also discussed.

Having been unable to procure Féré's work I am unable to appraise the technical control which he employed or to criticize the conclusions which James reports. It is no reflection on the original author to mention that exceedingly great care, a highly refined technique and a large number of measurements would be necessary to establish the dependence of the effects reported, on the wave-length of the radiation used as the stimulus. For our present purposes it is probably sufficient to point out that Féré's stimuli were discontinuous, of short duration, and were presented suddenly at or near a critical stage of each individual reaction. In these respects his conditions differ essentially from our own.

Watson[3] reports that when normal rats, previously trained to traverse a maze in the light, were required to repeat the performance in darkness, a considerable disturbance resulted. This he suggests may have been due to deprivation of the tonic effect of stimulation by light. "The effect of the light may have been general and stimulatory rather than specifically visual." When a very faint illumination was added the animals resumed their former activity and accuracy. He found, however, that rats which had been trained in the normal state and afterwards had undergone enucleation of the eyeballs exhibited a normal performance after the operation; and that rats untrained before the operation afterwards made normal records in learning the maze.

[3] Watson, John B. Kinaesthetic and organic sensations: their rôle in the reactions of the white rat to the maze. Psychol. Monogr., viii, whole no. 33. Princeton, N. J.: The Psychological Review Company, 1907.

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Dunlap[4] obtained the times required for a small number of simple reactions from four subjects under the following conditions, designated by the symbols in parentheses: to the snap generated in a telephone receiver by the passage of a spark-current through an induction-coil with which the receiver is connected (S); to the flask from a helium tube on a diffusing surface as a spark current was discharged through the tube (F); to the sound mentioned above, but synchronized with the flash (Sf); and to the flash just mentioned, synchronized with the sound (Fs).

Dunlap's statistical presentation being somewhat unusual and rather difficult of interpretation, I have summarized in table 1 of this report the data which he presents on p. 326 of the original article.

Two of the subjects, *W* and *D*, show a slight retardation of reaction when the flash is combined with the sound (Sf) as compared with the time required for reaction to the sound alone (S). The effect on these two subjects is sufficient to characterize the average for the group. The magnitude of the effect, however, is small, both absolutely and with respect to its probable error: so that, from such a small group of measurements one can hardly draw a conclusion of which one could feel highly certain.

The combination of the sound with the flash (Fs) shortened the time required for reaction as compared with the time required for reaction to the flash alone (F). The magnitude of the effect is large and its reliability is very high. From superficial inspection one would suspect that in some of the cases in the (Fs) series, the subject had reacted to the sound instead of the flash. Dunlap does not so interpret the results. His reasons are based in part on the subjects' descriptions of their adjustment to the situation, and partly on the results obtained from two reactors under two other compared conditions; in the first series (Fs-ds) of which two kinds of stimuli were presented: a flash accompanied by sound (Fs) with reaction to the flash, and sound alone, to which reaction was to be inhibited. In the second series

[4] Dunlap Knight (with the assistance of G. R. Wells): Some experiments with reactions to visual and auditory stimuli. Psychol. Rev., 1910, xvii, 301–318.

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(F-ds) were presented a flash alone (F) to which the subject was to react, and a sound alone, to which reaction was to be inhibited. Both subjects required a longer time for reaction to (F-ds) than to (Fs-ds), showing that the combination of sound with flash tended to shorten the reaction-time.

I confess to some misgivings over Dunlap's belief that this check experiment showed that in the original one, the subjects

TABLE 1

Summary of Dunlap's results

SUBJECT	SIMPLE SENSORY REACTIONS TO SOUND ALONE			SIMPLE SENSORY REACTIONS TO SOUND SYNCHRONIZED WITH FLASH			D_1	PE_{D_1}	$\frac{D_1}{PE_{D_1}}$
	P. R.	M_s	PE_{M_s}	P. R.	M_{sf}	$PE_{M_{sf}}$			
M	30–165	97.2	1.62	24–171	97.0	1.83	−0.2	2.45	0.08
W	23–211	117.3	2.59	34–215	124.6	2.34	7.3	3.49	2.09
D	51–162	106.3	1.34	31–192	111.2	1.93	4.9	2.34	2.09
J	15–196	105.1	1.87	20–191	105.5	1.75	0.4	2.56	0.15
General	13–202	107.4	0.97	10–210	110.0	1.01	2.6	1.45	1.80
SUBJECT	SIMPLE SENSORY REACTIONS TO FLASH ALONE			SIMPLE SENSORY REACTIONS TO FLASH SYNCHRONIZED WITH SOUND			D_2	PE_{D_2}	$\frac{D_2}{PE_{D_2}}$
	P. R.	M_F	PE_{M_F}	P. R.	M_{Fs}	$PE_{M_{Fs}}$			
M	116–202	159.5	1.07	43–192	117.3	1.80	42.2	2.09	20.2
W	77–269	173.2	2.48	40–237	138.3	2.53	34.9	3.54	9.9
D	95–246	170.3	1.84	38–196	116.7	1.89	53.6	2.66	20.1
J	105–217	156.4	1.08	24–202	113.2	1.83	43.2	2.15	20.1
General	77–249	162.9	0.88	31–220	120.4	1.01	42.5	1.34	31.7

By P.R. is denoted the probable range of the measurements, as indicated by Chauvenet's criterion; by M, the mean of those measurements falling within the limits of P.R. (a very few measurements were thus excluded); by P.E., the probable error of the determination denoted by the subscript; by D_1 the value of M_{sf}–M_s; and by D_2, the value of M_F–M_{Fs}. The ratio $D:PE_D$ gives a means of ascertaining the probability for and against the occurrence of D being due to chance. The values of P.R. are subject to an error of unknown magnitude, assumed to be of a secondary order, due to deviation from normality of distribution. It is assumed that the means are normally distributed whether the distributions of the measurements of which they are the means be normal or not. The unit of measurement is 0.001 second.

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did not sometimes react to the sound component instead of the flash-component in the (Fs) series. In the case of reactor *W* only thirty reactions in each of the two series in the check-experiment are compared. These indicate nothing contrary to Dunlap's hypothesis, the only ground for criticism being that the number is small. In the case of reactor *J* (the present writer) sixty reactions are presented under each of the two conditions. In the course of accumulating the sixty under condition (Fs-ds) which are presented, eleven reactions were made to the sound alone, to which reaction was to be inhibited. In the absence of conflicting evidence, one would suspect that approximately eleven reactions to the sound component were made when the stimulus was the flash accompanied by the sound; and that these reactions, being shorter than the ones made to the flash-component, tended to reduce the average for the series. Under condition (F-ds) only five errors were recorded.

In the circumstances I am inclined to suspension of judgment as to whether the second component in Dunlap's stimuli tended to reinforce, inhibit or usurp the place of the first component.

THE PRESENT PROBLEM

The specific problem attacked in the present study is whether tactile discrimination is increased, diminished or unaffected by uniform and continuous stimulation of the retina by light, under conditions which preclude the use of vision as a direct aid.[5]

PROCEDURE

Tactile discrimination can be indirectly measured by the speed and accuracy of performance in which it is an essential factor. This method was adopted for our present purpose. The task chosen was that of sorting a pack of 100 shuffled playing-cards of standard size into four "suits" of 25 cards each, according to patterns perforated in them, to be distinguished by the senses of touch alone.

[5] I am indebted to Professor Dunlap for suggesting the use of a card-sorting test in this problem. The patterns to be discriminated, and the procedure followed, are of my selection.

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The stimuli were prepared as follows: Four holes, about 4 mm. in diameter, were perforated in the cards, to form a quadrilateral whose diagonals lay vertically and horizontally and intersected in the center of the card. The figures thus formed were as follows:

1. A "large" square, whose diagonals were 50 mm.
2. A "small" square, whose diagonals were 30 mm.
3. A "wide" rhombus, whose vertical diagonal was 30 mm. and its horizontal diagonal 50 mm.
4. A "tall" rhombus, whose vertical diagonal was 50 mm. and its horizontal diagonal 30 mm.

The figures were designated by the adjectives enclosed in quotation-marks. The subject was instructed to "feel" each card separately and lay it in one of four piles conveniently spaced from left to right in the order given above, and to speak, subvocally, the name of the figure as soon as he determined it. The rhyme occurring in the names of the piles when arranged in order proved to be a useful mnemonic in the early stages of learning.

During all of the work, the subject wore a pair of special chauffeur's goggles, hinged at the nosepiece and with a covering of plush over the framework which fitted closely to the subject's face. The lenses were unusually large and covered the whole of the visual field when the subject's gaze was not extremely oblique. The lenses were thick and were sand-blasted so that they formed almost perfect diffusers. They were kept clean by the application of alcohol. It was impossible to distinguish any objects through them, or indeed to detect gross differences in brightness of objects placed farther than 20 cm. from the eye.

The subject was required to hold the head in an easy position, tilted slightly backward, with the muscles of the neck relaxed, and to look horizontally through the lenses toward a white-washed wall about 80 cm. from the eyes. The table on which the cards were to be sorted was also covered with heavy cardboard heavily sized with baryta.

The work was done under two compared conditions: (1) with the room in total darkness, except for a slight leakage about the frames of the openings in it; and (2) with the room lighted in such

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a way that as the subject gazed through the frosted lenses at the wall the entire visual field appeared as of nearly a uniform brightness of approximately 15 candles per square meter (varying somewhat if the position of the head were considerably changed) and having a distribution of energy among the wavelengths of the visible spectrum approximating that of a black body at 2600° K. Hereafter the first condition will be called *D* and the second condition, *L*.

At each sitting the subject was required to sort the shuffled pack four times: twice under each condition. Two orders of presentation were used in alternation: (a) *LDDL* and (b) *DLLD*. Half the subjects were given order (a) at the first sitting and the other half order (b). No consistent effects on the averages were discoverable.

The subject was informed that his performance would be rated on both speed and accuracy; that he would be penalized for mistakes sufficiently to overbalance any gain in speed which he might make at the expense of accuracy. He was instructed, however, not to expend time in correcting a mistake once made, as this would tend to double the penalty. He was also advised to try for accuracy at the expense of speed until perfect accuracy was attained, and was assured that the early acquisition of accuracy would tend to make for earlier and more rapid acquisition of speed. He was instructed as to the purpose of the experiment, but was informed that no reason existed for expecting a differential effect in favor of either condition; and he was urged to try equally hard for a maximum of achievement under both conditions.

THE SUBJECTS USED

Some information regarding the 16 subjects may be of interest. *E. S.* and *G. H.* were laboratory technical assistants, graduated from high school some two years previously. *F. G.* was the laboratory electrician. These three subjects were somewhat handicapped with respect to the others, by reason of calloused fingertips.

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E. M. was an undergraduate student in Case School of Applied Science with about one year of previous experience as observer in psychological work. He is slightly neurasthenic in type.

F. F. and *E. M. K.* were unmarried female stenographers of high school training.

L. T. T. and *H. M. J.* were psychologists, and *P. W. C.* a physiologist with long training and experience in psychological work.

L. D., *L. S.*, *H. D.* and *I. L.* were undergraduate students in the college for women of Western Reserve University, as was also *C. C.* *M. S.* was an unmarried woman of about 19, a high school graduate, not employed outside her own home. The two subjects mentioned last were reactors in another experiment[6] in the report of which they are designated as *C* and *M* respectively. In that report, *C. C.* is erroneously described as having exhibited in this experiment a better performance in the dark than in the light. The assertion was based, not on the original data, which I had not then seen for two years, but on a tabulation prepared by an assistant, who had inadvertently transposed the headings of two columns of scores. I take this occasion to make the correction.

G. P. L. was a physicist, with considerable experience in astronomical observation and in stellar photography.

The idiosyncrasies of certain subjects should be mentioned.

M. S. exhibited some emotional instability, in the direction of excessive timidity, aversion to taking the initiative, distrust of her own judgment, etc., which were occasionally excited by incidents of her engagement. She asserted both of this and the experiment mentioned above, that she felt slightly ill at ease in the dark, and enjoyed working in the light more. She was uncertain of her judgment of relative speed of work under the two conditions.

E. M. K. preferred the light, but reported that she could "concentrate better" in the dark, and thought her performance in the dark was superior. *E. S.* gave an almost identical report.

[6] Johnson, H. M.: The influence of the distribution of brightnesses over the visual field on the time required for discriminative responses to visual stimuli. This Journal, 1918, i, 459–494.

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H. M. J. preferred the light, but usually felt quite certain that his work in the dark was the more rapid, though requiring a greater effort. *G. H.* and *P. W. C.* gave similar descriptions.

I. L., L. S., L. T. T. and *C. C.* preferred the dark, and were certain that their performance was better in the dark than in the light. They thought that they were "less distracted" in the dark.

H. D. preferred the light, and was sure that her performance was better in the light than in the dark.

F. G., F. F., and *E. M.* had no preference, and their judgments of performance varied inconsistently.

G. P. L. was greatly prejudiced in favor of the dark. He habitually works in the photographic darkroom without using the ruby lamp, preferring to rely on touch alone than to utilize "unsatisfactory and inadequate illumination." He was certain at all times that he could work better without the distraction of "light that he couldn't see by," and predicted that his scores would justify his views. I think it unlikely, however, that he consciously failed to exert a maximal effort at any time; and as a comparison of the opinions of the other observers with their scores will show, one's personal estimates of time under the conditions of the experiment are quite unreliable.

Most of the subjects, it will be noted, believed their performance in the dark to be superior to their performance in the light.

RESULTS

The daily results are presented in tables 4 to 9, according to subjects. They are summarized for the several subjects in table 2. The performance of the group as a whole is shown statistically in table 3 and graphically in Fig. 1.

The character of the results necessitated their subjection to a somewhat unusual form of statistical treatment, in order to exhibit them adequately. A brief account may be of interest.

The subject's daily performance (P) under either condition is measured by the number of cards correctly sorted per unit of time. The unit chosen was 1000 seconds, for convenience of

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tabulation. This measurement establishes the time consumed in making an error as the measure of the penalty imposed. The penalty is arbitrary. In a factory, for example, the penalty for errors would depend on the relative value of the raw material with respect to the finished product; the value of the spoiled material as salvage; the time the machinery must be stopped to

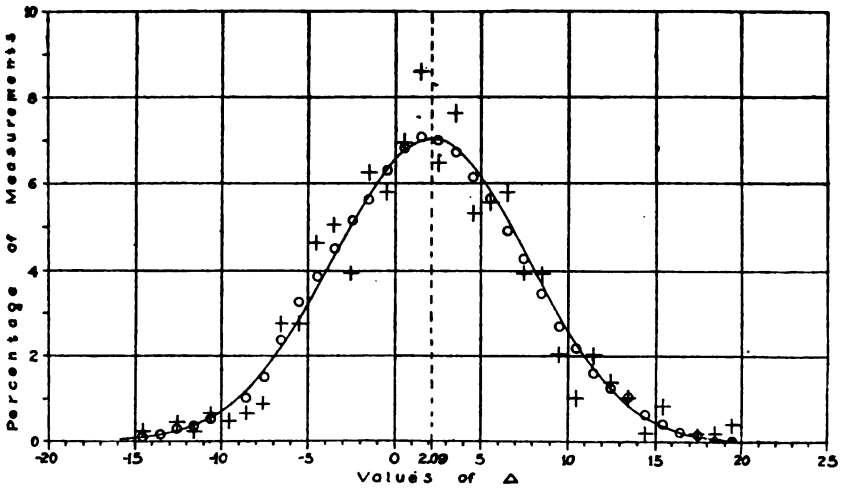

FIG. 1. PERCENTAGE DISTRIBUTION OF PERCENTAGE DIFFERENCES BETWEEN DAILY PERFORMANCE IN LIGHT AND IN DARKNESS, FOR ALL THE SUBJECTS (SEE TABLE 3)

Ordinates: percentage of measurements included in average. Abscissae: values of Δ. The difference (Δ) is expressed as a percentage of the performance in darkness, or

$$\Delta = \frac{100\,(P_L - P_D)}{P_D}$$

The empirical measurements are indicated by crosses. The circles indicate a "smoothed" distribution of the original measurements, obtained by one application of the "moving average" in groups of five. The curve represents the most probable distribution of a large number of similar measurements about the mean according to the Gaussian law.

correct errors, etc. In certain circumstances it would be profitable to increase speed at the expense of accuracy within certain limits; and these limits would be determined by such factors as I have mentioned and would vary in different shops. In this

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experiment there was no economic basis for assigning any given weight to errors.

Fortunately, the problem permitted the early acquisition of perfect accuracy. In very few cases did the errors exceed two per cent after three or four days of practice; while some subjects made only sporadic and single errors after the first day. The factor of speed therefore chiefly determines the measure of performance.

Each daily sitting yields two scores of performance: one for light (P_L) and one for darkness (P_D). The conventional method of treatment would be to average the P_L's and P_D's for each subject over the duration of the study and to compare the difference between the two averages with its probable error in order to establish its significance. This treatment, however, is appropriate only if the results tend to group themselves about a mean in accordance with the laws of probability. It is not appropriate if a large constant source of variation operates together with the fortuitous factors.

Such a condition is presented in the present case, due to the effect of practice, which in a few days' time multiplied the degree of skill exhibited in the initial stages of learning. Two alternatives were presented: (1) to train the subjects until the increase of skill from day to day became negligibly small, and discard the results obtained before this point is reached; or (2) to correct for the effect of practice. Considerations of economy necessitated the adoption of the latter plan.

The correction was accomplished as follows: Each P_L is diminished by the corresponding P_D, and the difference (Δ) is expressed as a percentage of P_D. In mathematical notation,

$$\Delta = \frac{100\ (P_L - P_D)}{P_D}$$

Then, for each subject, the average differential effect (M_Δ) is expressed by $\Sigma\ \Delta \div N$, in which N represents the number of averaged terms.

Practically the result was obtained simply by expressing each P_L as a ratio per cent P_D; averaging the ratios; and then dimin-

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ishing the average by 100. The remainder directly expresses M_{Δ}.

The distribution of these ratios was found to justify the application of the conventional methods of statistical presentation.

The number of measurements (N) being small for each individual subject, elaborate treatment was considered unnecessary, and the measures of variability and reliability were got by the use of approximate formulas. The distribution in each individual case was assumed to be normal; the mean variation was calculated by Dunlap's method;[7] and the probable error of the mean was taken as

$$PE_M = \frac{0.8453 \times MV}{\sqrt{N-1}}$$

The improvement during the first sitting being sometimes so large as to distort comparison between P_L and P_D, and as other sources of disturbance (such as illness, etc.) occasionally introduced questionable values, Chauvenet's criterion of exclusion was applied to the extreme measurements. The excluded measurements are shown in the tables but are omitted from the averages.

By reference to table 2 it will be seen that very important individual differences exist. Six subjects gave differences in relative performance in favor of the light, varying between 1.9 and 6.7 per cent. Being fairly large with respect to their probable errors, these differences may be considered statistically reliable. Seven subjects showed differences in favor of the light varying between 2.7 and 0.01 per cent, but these differences are so small with respect to their probable errors that they may reasonably be attributed to chance. That is, on the basis of the laws of chance, one would expect a reversal of the direction of the dif-

[7] Dunlap, Knight: Obtaining the mean variation with the aid of a calculating machine. Psychol. Rev., 1913, xx, 154–157. For correction of typographical errors in his formulas, and for an extension of the method, see Johnson, B. J.: Dunlap's method for the mean variation. This Journal, 1918, i, 325 f. Cf. also Whipple, G. M.: Manual of Mental and Physical Tests, i, 21 f. Baltimore, Warwick & York, 1914.

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ference once in a small number of repetitions of the experiment. Two subjects showed slight differences in favor of the darkness, the differences being statistically attributable to chance; while one subject, the astronomer, showed a difference in favor of darkness which would justify odds of 100 to 1 that repetition of the experiment would not result in a reversal of the direction of the difference.

TABLE 2

Summary of differences between the performance of the several subjects in light and in darkness

SUBJECT	M_Δ	PE_{M_Δ}	RATIO $M_\Delta : PE_{M_\Delta}$
E. M. K.	6.67	0.747	8.93
H. M. J.	6.45	0.307	21.00
M. S	5.51	0.604	9.14
C. C.	4.40	0.725	6.07
G. H.	2.88	0.571	5.04
E. S.	1.91	0.399	4.78
I. L.	2.72	1.38	1.98
F. G.	1.47	0.807	1.82
P. W. C.	0.56	0.552	1.02
H. D.	0.55	0.863	0.65
L. S.	0.44	0.558	0.79
L. D.	0.21	0.415	0.5
E. M.	0.01	0.721	0.01
F. F.	−0.01	0.534	0.02
L. T. T.	−0.41	0.984	0.42
G. P. L.	−2.07	0.549	3.77
All subjects.	2.09	0.184	11.4

Explanation: The difference in performance in light and in darkness for any particular day is given in per cent performance in darkness, and indicated by the symbol Δ. In other words:

$$\Delta = \frac{100\,(P_L - P_D)}{P_D}, \text{ in which}$$

P = number cards correctly sorted per 1000 seconds;

P_L = Performance in light, and P_D = performance in darkness, as thus measured; while M indicates the mean, and PE the probable error, of the determinations indicated by the subscripts.

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The low statistical reliability of a number of the averages included in the second group in table 2 is probably due to the small number of measurements. I consider the difficulty of getting a sufficient number of measurements to be one of the most serious objections which can be urged against the work-method as a means of exhibiting differential effects when the effects, as in the present case, are small.

The fact of individual differences being established, one may inquire whether the performance of the group of subjects, as a whole, was better in the light than in the dark, or the contrary. In other words, in case the whole population to which this group is a sample were experimented upon in groups of 16, what is the probability that the average performance of a group would be better in the light than in the dark?

These questions are met by including in one distribution all the Δ's of all the subjects and calculating the constants of the distribution. The average (M_Δ) and the standard deviation (σ) were obtained by the method of tabulation. Extreme measurements which failed to satisfy Chauvenet's criterion were excluded, and the probable error of the average (PE_{M_Δ}) was taken as $0.6745\ \sigma \div \sqrt{N}$. The distribution is shown in classes each having a range of 1 per cent, and each class is designated by the numerical value of the mean of the measurements included in it. For example, the class designated as 5.45 includes all the measurements between 5 per cent and 5.9 per cent inclusive and the value of each of the measurements included in the class is taken as 5.45 per cent.

The general distribution is shown statistically in table 3 and graphically in figure 1. The results indicate that the average performance of the entire group is 2.09 per cent better in the light than in the dark; and that the probability is of the order of 1×10^{-14} that the direction of the difference would be reversed by repetition of the experiment.

The form of the empirical frequency curve suggests that the deviations from normality of distribution are due to limitations of simple sampling.

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TABLE 3

Distribution of differences (Δ) *between daily performance in the light* (P_L) *and in the darkness* (P_D) *for all the subjects*

$$\Delta = \frac{100\ (P_L - P_D)}{P_D}$$

Δ CLASS (UNIT = 0.01 P_D)	NUMBER OF MEASUREMENTS
−14.55	1
−13.55	0
−12.55	2
−11.55	1
−10.55	3
−9.55	2
−8.55	3
−7.55	4
−6.55	12
−5.55	12
−4.55	20
−3.55	22
−2.55	17
−1.55	27
−0.55	25
0.45	30
1.45	37
2.45	28
3.45	33
4.45	23
5.45	25
6.45	25
7.45	17
8.45	17
9.45	9
10.45	7
11.45	9
12.45	6
13.45	5
14.45	1
15.45	4
16.45	0
17.45	1
18.45	1
19.45	2
N	430
M_Δ	2.09
PE_{M_Δ}	0.184
$M_\Delta \div PE_{M_\Delta}$	11.4

NOTE: Eight measurements, lying outside this range, were excluded from the average by Chauvenet's criterion. They are shown in tables 4–9.

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TABLE 4

Comparison of daily performance in light (P_L) *and in darkness* (P_D)

DAY	SUBJECT E. M. K.			SUBJECT H. M. J.			SUBJECT M. S.		
	P_D	P_L	$\frac{100 \times P_L}{P_D}$	P_D	P_L	$\frac{100 \times P_L}{P_D}$	P_D	P_L	$\frac{100 \times P_L}{P_D}$
1	117	154	131.6*†	106	125	117.9*	123	134	108.9
2	224	267	191.1	178	176	98.9*	193	188	97.4
3	255	278	109.0	168	182	108.3	228	234	102.6
4	246	268	108.9	181	220	121.5*†	242	243	100.4
5	256	265	103.5	229	244	106.5	269	251	93.3
6	262	287	109.5	261	278	106.5	259	263	101.5
7	280	303	108.2	265	284	107.2	259	261	100.7
8	309	347	112.3	225	248	110.2	247	264	106.9
9	302	332	109.9	255	270	105.7	293	280	104.6
10	314	364	115.9	252	270	107.1	296	290	98.0
11	384	377	98.2	266	289	108.6	267	297	111.2
12	357	403	112.8	268	279	104.0	289	310	107.2
13	382	415	108.7	280	292	104.3	292	318	108.8
14	388	420	108.2	288	306	106.2	295	305	103.4
15	397	414	104.3	317	328	103.5	293	322	109.9
16	420	448	106.6	299	314	105.0	309	331	107.1
17	437	417	95.4	295	317	107.4	339	346	102.1
18	431	445	103.2	333	354	106.3	279	316	113.2
19	455	455	100.0				307	336	109.4
20	442	491	111.1				311	334	107.4
21	474	508	107.2				303	337	111.2
22	485	506	104.3				308	304	98.7
23	476	476	100.0				307	310	101.0
24	462	488	105.6				318	353	111.0
25	456	518	113.6				298	329	110.4
26	489	504	103.0				321	338	105.3
27	504	487	96.6				340	381	107.0
28	513	542	105.0				356	376	105.6
29							398	429	107.8
30							393	406	103.4
31							354	441	124.6*†
32							409	454	110.0
33							405	432	106.6
34							419	420	100.2
35							397	452	113.8
36							417	411	98.6
37							414	425	102.7
38							443	438	98.9
39							437	522	119.4
40							462	504	109.1

* Excluded from average of individual's results by Chauvenet's criterion.
† Excluded from general average by Chauvenet's criterion.

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TABLE 5

DAY	SUBJECT C. C.			SUBJECT G. H.			SUBJECT E. S.		
	P_D	P_L	$\frac{100 \times P_L}{P_D}$	P_D	P_L	$\frac{100 \times P_L}{P_D}$	P_D	P_L	$\frac{100 \times P_L}{P_D}$
1	157	174	110.8	97	148	152.6*†	118	128	108.4*
2	237	213	89.9	194	195	100.5	203	198	97.5
3	236	239	101.2	205	219	106.8	235	243	103.4
4	274	255	93.1	231	235	101.7	294	305	103.7
5	292	328	112.3	229	242	105.6	322	320	99.4
6	327	312	95.4	274	275	100.5	367	379	103.3
7	332	315	94.9	250	282	112.8	402	409	101.8
8	329	346	105.2	291	276	94.9	384	405	105.4
9	331	346	104.6	296	298	100.6	392	412	105.1
10	358	352	98.4	301	300	99.7	459	462	100.6
11	377	425	112.7	305	306	100.3	488	501	102.6
12	399	427	106.9	319	308	96.5	495	499	100.7
13	383	415	108.5	304	352	115.8*	509	517	101.6
14	439	424	96.6	334	351	105.1	510	518	101.6
15	411	433	105.4	343	370	107.9	510	510	100.0
16	443	426	96.2	352	339	96.3			
17	423	490	115.8	343	357	104.1			
18	479	489	102.2	343	350	102.0			
19	475	511	107.6	365	375	102.7			
20	495	509	102.8	343	365	106.4			
21	471	509	108.1	363	391	107.7			
22	518	506	97.7	374	382	102.1			
23	509	578	113.5	379	377	99.5			
24	482	541	112.1	404	395	97.8			
25	570	545	95.7	380	425	111.8			
26	614	618	100.6	411	428	104.1			
27	553	637	115.3	382	411	107.5			
28	551	494	89.7	401	434	108.2			
29	505	597	118.2	404	392	97.1			
30	614	608	99.1	421	428	101.6			
31	606	644	106.1	434	441	101.6			
32	654	678	103.7	447	445	99.5			
33	643	704	109.5	458	474	103.5			
34	641	669	104.4	446	477	106.9			
35	680	705	103.6	458	440	96.1			
36	678	690	101.7	440	446	101.3			
37	596	652	109.5	430	458	106.5			
38	669	695	103.8	431	459	106.5			
39	702	747	106.4						
40	676	683	101.0						
41	702	730	104.0						
42	747	855	114.5						
43	715	791	110.6						

* Excluded from average of individual's results by Chauvenet's criterion.
† Excluded from general average by Chauvenet's criterion.

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TABLE 6

Comparison of daily performance in light (P_L) and in darkness (P_D)

DAY	SUBJECT I. L.			SUBJECT F. G.			SUBJECT P. W. C.		
	P_D	P_L	$\frac{100 \times P_L}{P_D}$	P_D	P_L	$\frac{100 \times P_L}{P_D}$	P_D	P_L	$\frac{100 \times P_L}{P_D}$
1	‡	‡	‡	126	97	75.9*†	146	154	105.5
2	108	116	107.4	171	184	107.6	160	153	95.7
3	141	144	102.1	205	194	94.7	186	188	101.0
4	163	156	95.7	219	238	108.7	196	193	98.5
5	185	191	103.3	276	276	100.0	218	222	101.8
6	210	233	110.9	336	319	94.9	225	210	93.4
7	210	232	110.5	321	348	108.4	232	236	101.7
8	214	218	101.8	325	343	105.5	238	243	102.1
9	232	238	102.6	351	353	100.5	244	218	89.4*
10	242	227	93.9	378	360	95.2	228	241	105.7
11	262	250	95.5	354	355	100.3	225	226	100.5
12	258	246	95.4	388	388	100.0	245	243	99.2
13	259	294	113.5	404	399	98.8	260	261	100.4
14				399	417	104.5	256	242	94.6
15				388	431	111.1	249	249	100.0
16				403	424	105.2	252	265	105.1
17				429	453	105.6	258	257	99.6
18				455	423	93.0	287	282	98.3
19				453	470	103.7	264	287	108.7
20				447	429	96.0	266	262	98.5
21				449	431	96.0	287	280	97.6
22				397	408	102.7	279	290	103.9

* Excluded from average of individual's results by Chauvenet's criterion

† Excluded from general average by Chauvenet's criterion.

‡ A portion of the record for this day was lost.

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TABLE 7

Comparison of daily performance in light (P_L) and in darkness (P_D)

DAY	SUBJECT H. D.			SUBJECT L. S.			SUBJECT L. D.		
	P_D	P_L	$\frac{100 \times P_L}{P_D}$	P_D	P_L	$\frac{100 \times P_L}{P_D}$	P_D	P_L	$\frac{100 \times P_L}{P_D}$
1	112	117	104.4	171	162	94.7	144	139	96.6
2	175	170	97.2	222	213	96.0	152	153	100.6
3	222	235	105.9	252	240	95.2	216	215	99.5
4	224	222	99.1	254	261	102.8	288	286	99.4
5	274	293	106.9	288	272	94.5	302	309	102.3
6	281	280	99.7	301	298	99.0	342	327	95.6
7	310	296	95.5	354	338	95.5	343	346	100.9
8	376	356	94.7	386	394	102.1	364	376	103.2
9	395	389	98.5	405	382	94.4	403	399	99.2
10	385	383	99.5	394	399	101.2	357	378	105.9
11	377	420	111.4	415	414	99.6	389	386	99.3
12	406	435	107.2	456	487	106.8	437	451	103.1
13	431	417	96.8	487	481	98.7	495	494	99.8
14	438	423	96.6	501	496	99.0	488	474	97.1
15	484	483	99.8	505	509	100.7	512	505	98.7
16	450	459	101.9	524	547	104.3	528	536	101.5
17	482	480	99.6	525	547	104.2	460	431	93.8
18	474	481	101.5	565	585	103.5	461	482	104.5
19	482	509	105.6	582	614	105.5	513	463	90.3*
20	463	455	98.3	603	631	104.6	519	534	102.8
21	435	464	106.6	654	667	102.0	499	504	101.0
22	503	514	102.1	654	685	104.7	534	514	96.3
23	511	531	103.9	625	685	109.6	494	479	97.0
24	562	546	97.2	685	717	104.6	480	506	105.4
25	547	556	101.6	678	683	100.7	505	480	95.0
26	528	504	95.5	705	705	100.0	520	514	98.9
27	514	541	105.2	736	709	96.3	512	538	105.0
28	522	541	103.6	741	727	98.1	544	529	97.3
29	562	565	100.5	738	772	103.2	500	514	102.8
30	556	702	126.2*†	837	766	91.6	538	548	101.8
31	733	599	81.7*†				506	528	104.2
32	599	616	102.8				534	524	98.2
33	646	606	93.9						
34	728	689	94.6						
35	676	577	85.4						
36	627	663	105.7						

* Excluded from average of individual's results by Chauvenet's criterion.
† Excluded from general average by Chauvenet's criterion.

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TABLE 8

Comparison of daily performance in light (P_L) *and in darkness* (P_D)

DAY	SUBJECT E. M.			SUBJECT F. F.			SUBJECT L. T. T.		
	P_D	P_L	$\frac{100 \times P_L}{P_D}$	P_D	P_L	$\frac{100 \times P_L}{P_D}$	P_D	P_L	$\frac{100 \times P_L}{P_D}$
1	116	123	106.0	166	146	88.0*	176	163	92.6
2	114	95	83.3*†	219	234	106.8	277	243	87.8
3	142	158	111.3	279	263	94.3	258	263	102.0
4	177	171	96.7	287	309	107.6	298	291	97.7
5	202	199	98.5	354	347	98.1	314	326	103.8
6	233	216	92.8	372	359	96.6	327	324	99.1
7	237	241	101.7	374	384	102.7	326	347	106.4
8	266	275	103.4	392	379	96.7	343	352	102.6
9	270	237	87.8*	432	409	94.7	341	349	102.3
10	295	304	103.1	441	443	100.4	369	366	99.2
11	288	291	101.0	421	418	99.3	330	352	106.7
12	310	291	93.9	467	436	93.4	373	382	102.4
13	320	293	91.6	475	470	98.9	438	403	92.0
14	322	328	101.8	471	487	103.4	415	430	103.6
15	321	320	99.7	513	518	101.0	406	420	103.5
16	293	319	108.9	485	527	108.6	404	410	101.4
17	316	299	94.7	561	536	95.6	445	407	91.5
18	319	325	101.8	550	569	103.4	426	409	96.0
19	321	325	101.3	536	511	95.4	432	438	101.4
20	335	351	104.7	566	587	103.7	408	433	106.1
21	324	321	99.1	588	566	96.3	454	423	93.2
22	371	365	98.4	549	568	103.5			
23	354	369	104.2	586	578	98.8			
24	343	348	101.4	574	559	97.4			
25	354	342	98.0	599	601	100.3			
26				596	580	97.4			
27				611	618	101.1			
28				556	582	104.7			
29				565	550	97.4			
30				566	572	101.1			
31				580	587	101.2			
32				559	597	106.7			
33				599	559	93.3			

* Excluded from average of individual's results by Chauvenet's criterion.

† Excluded from general average by Chauvenet's criterion.

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TABLE 9

Comparison of daily performance in light (P_L) *and in darkness* (P_D)

DAY	SUBJECT G. P. L.		
	P_D	P_L	$\frac{100 \times P_L}{P_D}$
1	129	142	110.1*
2	233	211	90.6
3	236	233	98.8
4	290	298	102.7
5	328	307	93.6
6	318	319	100.3
7	306	292	95.4
8	341	330	96.8
9	385	380	98.7
10	438	419	95.7
11	474	487	102.7
12	441	428	97.1
13	437	414	94.8
14	426	440	103.3
15	429	419	97.7
16	457	448	98.0
17	472	455	96.4
18	503	465	92.5
19	492	472	96.0
20	503	512	101.8
21	520	526	101.1
22	521	534	102.5

* Excluded from average of individual's results by Chauvenet's criterion.

The daily results obtained for the several subjects individually are shown statistically in tables 4 to 9 inclusive. The small number of observations made on some of the subjects is due to their lack of availability; and the irregular rate of improvement with practice, found in some cases, is due to unavoidable interruptions, most of which were occasioned by illness of the subjects.

DISCUSSION

The experiment shows that a minority of the subjects, and the group as a whole, gave a better performance in the light than in darkness. As the greatest portion of time and effort involved in

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the performance of the task is expended in the act of discriminating among the "suits" by the senses of touch, the inference seems justified that tactile sensitivity is enhanced by uniform stimulation of the retina by light, even though the visual and tactile impressions cannot be referred to the same object, and vision cannot serve even as a means of orientation.

Of the several hypothetical explanations which are possible, I shall mention only the one which seems to me to be the most useful.

Let us first admit the postulate that a differential response of some system of muscles is as essential a part of the process of perception, or of discrimination, as is the excitation of a nerve-center, or even the stimulation of a receptor. Then, any condition which tends to increase the tonicity of the muscles, or in other words, renders them more ready to contract when stimulated, will tend to lower the threshold and shorten the time required for perception and discrimination.

Such an increase in tonicity is actually brought about by the action of adrenalin, and probably by other products of internal secretion of whose manufacture little is now definitely known.

A similar result would ensue from the action of a similar product which might tend to sensitize the synapses.

Conversely, one might attribute the diminished sensitivity in darkness to the action of some endocrinal product which tends to diminish the tonicity of the muscles, or to retard the activity of the synapses, and which is produced in greater quantities in darkness than in light.

Either of these hypotheses would explain in terms of strictly physiological concepts the differential effects which were obtained, as the poorer performance in darkness appears to be due specifically to the absence of light and not to elimination of vision of objects.

The production of either of these hypothetical substances may be regarded as a conditioned reflex, occasioned by long habituation to activity in the light and a minimum of activity, tending toward somnolence, in the dark. That this habituation is capable of modification by training is suggested by the record of

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G. P. L., the astronomer (table 9); and also by comparison of the latest stage of performance of subject *L. S.* (table 7) with the preceding stages.

As to the dependence of the dynamogenic value of light on wave-length, intensity, or on the distribution of either or both: it would certainly seem that the question is open and important. The present writer feels, however, that the work-method is not the most appropriate one to its solution. The direct measurement of thresholds or of the time required for perception or for discrimination under the conditions which one may wish to compare, permits of the accumulation of an enormously greater number of measurements in the same period of time than can be obtained by the work-method. As the effects sought can not be readily isolated from those of other agencies, they can best be demonstrated by methods the results of which accumulate rapidly and lend themselves to statistical treatment.

In conclusion, I wish to acknowledge indebtedness to Dr. Buford J. Johnson for a critical examination of this report in manuscript; and for suggestions which materially improved the last section. This of course does not imply that she assents to the entire passage.

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INTEGRATION OF MOVEMENTS IN LEARNING IN THE ALBINO RAT

A STUDY OF THE ADJUSTMENT OF AN ORGANISM TO AN ENVIRONMENT

JOHN LINCK ULRICH

I. INTRODUCTION

It is usually said or is implied, that to accomplish the solution of a problem, an animal displays an effort, or does something definite to some part or other of the problem. An interpretation of this effort, or of how the animal comes to solve the problem, is based upon the way this effort is regarded. The idea of effort is always kept in mind by most investigators of animal behavior, when weighing the results of their observations. Either the effort shown by the animal is regarded as an evidence of intelligent behavior, and the results produced as the outcome of purposeful, self-directed, remodeled behavior; or the effort displayed is looked upon as purely mechanical, and the result of sensory excitations. In whatever way the effort displayed in an animal is viewed, it is evidenced in the production of a number of movements, one, or a group of which eventually solves the problem, and this last movement, or series of movements, is pronounced successful in contradistinction to the unsuccessful movements. This classification has almost invariably been the one used when considering the movements of an organism. Usually the recurrence of the successful movements is supposed to produce a change, or a reintegration in the nervous system, in some specific part to establish one common path for the production of these movements.

Explanations of learning in animals have therefore to a large extent been dependent upon the working concept of "trial and error," and with that which is inseparably bound up with it; namely, a division of all movements into successful and unsuc-

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cessful. Not until the successful or unsuccessful movements have been performed, can it be judged which movements of the number made are the most important. The unsuccessful kind are regarded as of little importance and of little consequence in learning, as compared with the successful. The latter alone are supposed to have an enduring effect. Since the unsuccessful movements do not disappear immediately, this division of all movements may be said to exist till the successful alone remain. Such an antithetical division of all movements based upon the working concept of "trial and error" must in no wise be considered a physiological classification of integrated movements; nor can it approach such a classification, for it is rather a personal estimate of movements.

Though in this twofold division of movements, the unsuccessful are regarded of less importance in learning, the fact remains that the relation of these to the successful must be shown. Supporters of intelligent effort of animals have usually combined with the antithetical concept another concept, pleasure-pain. In antithesis to pain, a pleasure-giving quality produces retention or fixation of successful movements, and in antithesis to pleasure, pain eliminates the unsuccessful kind. The successful movements are fixated, or stamped in, supposedly, because either pleasure is derived at the time of the performance of them, or arises soon after when reward is obtained. In any case it must be present in its most positive form at the time when it is in contrast to previously unpleasant or painful "sensations." When pleasure arises at the time of success, the contrast is supposed to be immediate, and an associated physiological process, such as an increase in bodily tone, a higher metabolic rate, occurs at the time of stamping in the successful movements. When pleasure is supposed to come after the act, that is at the time of reward, the contrast to pain evidently is not immediate with the performance of the successful movement, but is established later. If this is true, then what is painful must include everything before reward, inclusive of the final act itself, which can scarcely be painful unless pleasure is an anthropomorphic agent, capable of selecting the pleasurable-successful from the painful-unsuccessful movements.

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Moreover, the physiological increase in bodily tone is not synchronous with the successful act, but occurs when pleasure arises. Since physiological processes are closely correlated, it is difficult to see how this may be effected. On the other hand, it is stated that pain eliminates the unsuccessful movements. It appears that since pleasure and pain determine the different qualifications for movements, one kind of movement is set over against the other, and their relative importance established and regulated. Movements become thus, in some way or other, of consequence to the animal, and no physiological process of "stamping in" the successful kind occurs until this consequence is realized.

This consequence is of course established through sensory channels, and it must be the pleasurable sensory associations that produce this entire effect upon the organism. Often it is supposed that the successful movements are of such consequence to the animal, that there arises a protreptical tendency for their recurrence and repetition, and even for their selection.

The actual difficulty of arriving at a satisfactory view of the pleasure-pain concept is due to the simple reason that the antithetical terms, pleasure and pain, are inapplicable to any organization in an animal, and these concepts must, as a consequence, be inappropriately applied to successful and unsuccessful movements which can be said to have an organization in reflex mechanisms for their production. The relation and importance of the pleasure-pain concept for anything like the fixation of one kind of integrated movements, remains as an *a priori* assumption, so long as this pleasure-pain concept cannot be applied to any known organic process, such as hunger for instance, where an organization for its arousal exists in the structure and movements of the alimentary canal. Accordingly any attempt to prove the relation of the kinds of movements must ever involve the proof of this assumption. The terms pleasure and pain are thus hypothetical terms beyond the reach of scientific method; that is, to prove experimentally that pleasure, as such, does, or does not play a part in the fixation of successful movements in learning, apparently cannot be done.

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It is accordingly not a bit surprising that many modifications of the original working concept of trial and error and pleasure-pain in learning have been made. Such modifications, after introducing additional physiological explanations, have either discredited the concept of "pleasure-pain" or, have modestly retained it. These other explanations throw the burden of fixation of the successful movements partly or wholly upon the nervous system. With the occurrence of the successful movements, physiological changes of importance are supposed to result in a selection of pathways, or in a differentiation of them in the central nervous system. Sensory excitations, which were at first sent diffusely through many channels producing many movements of a "random" kind are, in consequence of the performances of the successful act, conducted, in the main, through one channel or common path. Such views state that either originally no preformed paths were present, or that reflex arcs were not selected, or that no passage through definite synapses, or reflex arc, existed as an outlet for a definite sensory excitation to produce the successful movement. These paths or passages are established in a degree when a successful act is once performed. Then definite pathways, through opposite (hypothetical), semi-impermeable membranes, the synapses, are established, by the reduction of the resistance through them for excitation, and this resistance lessens with a repetition of the successful act; or, definite pathways, and reflex arcs are selected or established because of a repetition of this same act. "Pleasure-pain" is, or is not, regarded as of significance in the fixation of either of the pathways, or in the establishment of reflex arcs.

In accordance with an idea of the existence of different degrees of development of reflex arcs or pathways in the nervous system, sometimes it is said that learning, leading to habit formation, occurs in two ways. Learning is rapid, immediately effective, when groupings of instinctive responses and successful movements soon establish a habit; it is slow, laborious, ineffective for a time, when instinctive responses result in the production of "random unconnected diffuse responses," and it requires the appearance of the successful act which by a longer repetition

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of the act leads to the establishment of a habit. The successful act in the first instance appears soon, and in the second, later, after successive efforts, with the production of numerous movements. When habit formation occurs in the first way, the reflex arcs, or the permeability of the synapses is already established; when occurring in the second way, these paths must be developed from the beginning of learning, or with the appearance of the successful movements. The supposition that in the last way learning is established through the performance of "random movements" is a logical position to assume, for sensory excitations are diffuse, until one excitation produces "successful" movements. Moreover, when habit is established in the first way, through the grouping of instinctive responses, nothing radically new is acquired; but in the second way, an actual modification in the nervous system is supposed to occur. There appears to be no experimental evidence for such a distinction in the establishment of habits.

In most of these theories, something more definite is supposed to occur in the organism than is stated when "pleasure-pain" alone fixates the successful movement, namely, a supposed, real modification within the central nervous system. When pleasure now accompanies sensory excitations with the performance of the successful act, instead of its increasing in general, the bodily tone of the animal, and fixating a movement, it helps to establish definite paths in the nervous system for this movement. When the "pleasure-pain" concept is omitted, repetition of the successful movement supposedly accomplished something more definite. Although in both instances much is gained by an increase in definiteness of the results supposedly accomplished, it is at times difficult to say whether the sensory excitations or the responses themselves produce on the organism the effect said to take place In all probability when it is said that "pleasure" is a factor of some consequence, sensory excitations have much to do with the establishment of definite paths or neural arcs in the nervous system, for "pleasure" is supposed to accompany excitations in the senses. When mechanical repetition of the successful act is said to increase the permeability of the synapses or

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establish neural arcs, sensory excitations appear again to be primarily of importance, not the responses themselves; for excitations in the external senses originally select or establish paths, or reflex arcs, through definite regions of the nervous system. Accordingly, all such theories are fundamentally based upon the establishment of "sensory associations," or "sensory-motor connections," and the repetition or recurrence of familiar successful stimuli are just as important as the repetition of the successful act or movements. Learning, then, is a question of obtaining successful stimuli, or of obtaining successful excitations, so that the successful movement can take place, and the unsuccessful stimuli, or unsuccessful excitations, may not produce unsuccessful movements. Thus, in the organism there must in all cases be established exact "sensory associations."

On the principle of the repetition of some definite movement have resulted theories of the kind generally outlined above. These theories have developed along the same line of reasoning, and start with the assumption of the existence of an antithetical division of movements. These theories are of interest because of their relation to traditional psychology.

The explanations of learning offered by Carr (1) supplemented by Watson (2) show a transformation of the original working concept of "trial and error" and "pleasure-pain" with the traditional division of all movements into successful and unsuccessful with which this concept is inseparably bound. Both these investigators disregard the "pleasure-pain" concept and start their theories with the usual acceptance of an antithetical division of all movements into successful and unsuccessful. The concept of "trial and error" still remains in its very foundation, for the "successful" movement is selected or produced when a great many movements are made.

According to Carr it is the recency, the frequency, and the sensory intensity of the final or the "successful" act or movements that "stamps in" or fixates these movements. The exact nature of these movements is left to conjecture. What is evidently produced by the recency and the frequency of the final act is an increase in sensory intensity or sensory excitations. The "suc-

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cessful" movements become, then, of some sensory consequence, instead of some pleasurable consequence, to the organism. Rather the opposite view is expressed by Watson. In his mechanistic conception of learning, the recency and the frequency of the performance of these movements alone produce the fixation, the retention, of them. The "successful" movements are definite movements which must be integrated to produce a unitary act. Sensory excitations, by selecting neural arcs, produce a "tying together or integration" of these movements. It is obvious that Watson does not wish to imply that these movements may produce any increase in sensory effect, and thereby weaken his own mechanistic explanation of the term repetition; and Carr, in his belief in an increase in specific sensory intensity, leaves open for suggestion the appearance of a "need." Since the pleasurable tendency is disregarded, the "need," the "psychic," comes into existence for the production of the "successful" movements at the moment the first specific stimulus is received.

In both these theories, in whatever manner the "successful" movements are retained, the "unsuccessful" are eliminated, disposed of, because they have no sensory consequence, or they are not repeated. Then in two different ways the "unsuccessful" movements may be eliminated. For Carr it is the intensity of the sensory impressions brought about by the recency and the frequency of the "successful" act that results in the establishment of the permancy of this act and eliminates the "unsuccessful" movement. For Watson it is the recency of the performance of the "successful" act with its supposed immediate physiological effect, coupled with the frequency or repetition and its cumulative physiological effect by which certain neural arcs become operative in producing this act, fixating it, that eliminates "unsuccessful" movements. One sort of movements is set over against the other, so that the "successful" movements in one case are of some sensory consequence to the organism, and in the other of no sensory consequence whatsoever, but of some physiological importance, and all the other movements are of indefinite value.

As in previous theories of the kind on learning, the burden of retention of movements is thrown on the nervous system. The

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explanations given above are based upon the establishment of "sensory associations" of one form or another. In one instance, increased sensory intensity can be brought about by establishing an association of the senses *by the production* of the "successful" movements and, in the other, definite sensory motor arcs can, in an orderly way, be associated *to produce* the successful movements. Some apparent differences may be thought to exist between the two explanations, for the first may be said to result in the establishment of "sensory associations," and the second in the establishment of mechanical sensory-motor connections. However, in reality, they appear to be the same, for sensory excitations of a specific kind produce the "successful" movements. It is the repetition of the "successful" act in both these explanations of learning, which establishes "sensory association" for that act, and, conversely, it is the repetition of the same sensory excitations that produces repeatedly the "successful" movements. In this theory emphasis on the repetition of an act with its ultimate result, is clearly the substitute for pleasure-pain for the retention of, or fixation of an act. One theory has the appearance of being a mathematical psychological explanation, and the other that of a mathematical physiological explanation for learning.

Other modified forms of the "trial and error" concept of learning exist. Holmes (3) offers as a substitute for "successful" and "unsuccessful" movements other antithetical terms, congruous and incongruous, in explanation for learning. The only thing that is gained by introducing these terms is, that they emphasize the striking position of two sorts of movements, bringing their relation to the fore and removing any necessity for introducing the "pleasure-pain" concept, with its incompatibility with a physiological explanation for the fixation of movements. The congruous movements bring solution to the problem and become associated with other instinctive acts of a congruous kind, and the "incongruous are discontinued, because they are instinctive acts of an incongruous kind, antagonistic to the right reactions, the congruous." Linking up of the "congruous" is accomplished by reason of the existence of organic congruity of

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the reactions reinforced by inhibition of the incongruous. No physiological explanation other than what is implied by associative memory fixates the "congruous" and inhibits the antagonistic incongruous kind. It cannot be otherwise than that "sensory association" is a selective agency acting upon the mechanism of adaptive, suitable, instinctive responses. An idea of harmony pervades the theory and there does not exist that inharmonious division of movements into "successful" and "unsuccessful" kinds. This theory is formulated in the expectation of discovering or introducing a substantial basis for "intelligence," and nothing need be added to a physiological explanation to introduce this idea even for the existence of primitive "intelligence."

Still another theory of learning is that in which the predominant influence of a stimulus is supposed to produce forced movements or tropisms. Loeb (4) is the exponent of this view. No "random" movements are assumed to be made by an animal, and as a result, no selection of movements need be made, and no diametrically opposite or antithetical kinds are supposed to exist. A distinctive type of movement, based upon an animal's organization, is forcibly brought to the fore by a stimulus. Learning however, is manifest only in organisms which show evidence that movements can be linked together by associative memory. What is meant by associative memory is not quite clear. Many investigators in animal learning, when they consider Loeb's descriptive account of associative memory, assume that here exists a criterion for "intelligence." Loeb seriously objects to such an interpretation of associative memory, but any interpretation is justifiable, for there is every evidence that by associative memory is meant that in learning "sensory association" is established. Recently he has made a more restricted but hardly a more satisfactory statement of what is meant by associative memory. "Learning is possible where there is an organ such as the brain for the establishment of associative memory, the mechanism of which is still unknown." Evidence of its existence is placed in the mechanism of the "conditioned reflex." It is similar to such a reflex. The "conditioned reflex" is usually regarded as a form

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of "sensory association," so that the mechanism for "associative memory" is in reality the establishment of "sensory association." There remains practically no difference in the two forms. One redeeming feature in this theory of learning is, that movements are dependent upon the existing organization of an animal, not upon modifications of structure in the nervous system.

With the persistence of the idea of "sensory association" in learning and with the acceptance of the doctrine of forced movements or of a twofold division of movement into the "successful" and the "unsuccessful," or the "congruous" and the "incongruous," there remains as a result little that is strikingly new or radically different, in all these theories of learning in animals. Some of them are formulated with the view of removing one objection, or all objections, to the existence of the "psychic," or "pleasure-pain." Into these different theories, when supporting or discrediting one view or another, supposed physiological explanations of a certain kind have been freely introduced. All of them cling to the basic supposition that a "sensory association" is brought about. In one theory it is conditioned by the performance of forced movements; in the other by the existence of antithetical movements, either with or without the influence of "pleasure-pain." All these theories are based upon sensory analysis and show the influence of analytical psychology.

As with all theories developed by analytical methods, these fail to consider matter that may interfere with the main body of their thesis. Mainly the "successful" attainment of a feat, the appearance of the "congruous" movement in learning is considered and little attention is paid to failure. Upon the occurrence of the "successful," or the "congruous," and the persistence of either, or upon the production of a directive type of movement, all these theories are dependent. Scarcely anything is said of the positive influence that other movements may have or of the significance of a failure to solve a problem or to learn it. Is it supposed that when the "successful" or the "congruous" movement is not performed, that the mechanism for the production of these movements is wanting? Is it assumed that certain neural arcs are absent or undeveloped, that sensory exci-

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tations cannot pass through the synapses, or that the mechanism for the production of the "congruous" movements is undeveloped, and all movements are "incongruous," or that when forced movements do not lead to the solution of a problem an organized mechanism for their production is either absent or functionally undeveloped? In all these theories an organization for their production must be assumed to be present, since learning in all instances is dependent, primarily upon the production of sensory excitations. Possibly when an animal fails to solve a problem no consideration need be taken of the conditions that produce failures; it is sufficient in any theory of learning to consider only positive results.

It is evident that any theory of learning which would consider all movements performed by an animal of physiological importance would be at variance with existing theories of learning; with such ideas as the production of forced movements, or the necessity of assuming a twofold division of movements and the insistence of a form of "sensory association" for the retention of the movements that solve the problem. It is our purpose in this paper to show by an investigation of the integrated movements in learning in the white rat; (1) that learning begins from the moment the rat is placed in the problem for preliminary feeding; (2) that all movements at all times are of equal importance in solving a problem, and that they are fundamental, inherent, integrated movements conjoined, or coördinated; (3) that several conjoined movements are used in an exaggerated manner to solve a problem, and a greater development of interaction of body parts is necessary for an exaggerated performance of these movements; (4) that no definite movements are retained, but, through training, these movements are performed with greater facility as is the case with any exercised part of an animal's body; and (5) that solution of a problem does not occur because an animal responds "successfully" after making repeated "efforts" to do something.

Any arbitrary division in this investigation of the integrated movements necessary to learning, would be quite out of place, since no component of all movements produced by a rat can be

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said to be "stamped in," fixated, or be of some consequence to the animal. The present investigation must determine whether the sorting of movements into antithetical sorts is justifiable, and whether, by so doing, the real difficulty in formulating a physiological explanation for learning arises. It is necessary to determine, if possible, a factor other than "sensory association" for the so-called retention of movements. It is evident that the most important consideration we are to face is the establishment of the relative position of every movement an animal makes before the solving of and during the learning of a problem. Unless observation can throw light upon the relation of all movements, the prevalent antithetical division of all movements must remain the very foundation of our theories of learning, and likewise the establishment of "sensory association" or even "pleasure" must still be considered a factor in the supposed fixation of movements.

II. THE LATCH-BOX PROBLEM

For the investigation of learning, several problems were selected, not so much in accordance with a preconceived plan, but as investigations proceeded some things unrevealed in one problem necessitated the continuation of the work with other problems. The first problem selected, the latch-box, showed limitations in respect to the number of solutions, though in a great number of rats learning took place. On the other hand, in the inclined-plane, and in the maze problems, solutions could be easily accomplished, but the number of rats in which learning could be facilitated was not great. The possibility of the external senses functioning to a greater extent in some problems than in others, led to the use of problems of "discrimination" and of delayed reaction.

A foremost consideration in the selection of problems was to establish the importance of every movement made while learning. To this end an understanding of the way any problem is solved is dependent upon a knowledge of the range of individual characteristic movements made by an animal. Again, an understand-

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ing of the nature or of the possibilities of any problem rests upon the same factors. Accordingly, care must be taken in selecting the problem as well as the animals used. It is necessary in this work to select animals that can be had in sufficient numbers, so that a given observation of any or of all movements can be repeated in order to establish their relationship, their constancy, and their importance. Furthermore, to have animals of a convenient size is an essential condition, for in larger animals are produced many lesser movements unfavorable for a given observation. No animal seems to fulfill these conditions, and appear better fitted for experimentation than the white rat.

The selection of the first problem revealing in a small space all the movements made by the rat and offering the greatest opportunity for the investigation of integrated movements would be of one producing movements of simplicity and of definiteness. No other problem seems to be as suitable in revealing these qualities as the latch-box problem. Accordingly, it is the first one selected for the investigation of learning of integrated movements. The only movements necessary for the solution of the problem are progression to the latch, and the simple act of lifting the latch out of the notch by the raising of the head. These integrated movements involved are sufficiently definite and constant, and are inherently conjoined or coördinated. Preliminary work with several rats established these important points.

The latch-box (fig. 1) was made entirely of metal. The box frame was constructed of 1.5 cm. square rod of aluminum. The box was 25 cm. square with a door 6 by 10 cm. in the lower center of one side. Within the sides, the top and the door were fitted lightly constructed frames covered with 25 cm. wire mesh. The door was closed by a latch, which was snugly held by the pressure of a close spiral spring attached to the inside of the door and to the opposite side of the box. When the latch was released, this same spring gently opened the door. The problem box was covered with a wire hood with plate glass covering part of one side, so that the behavior of the rats in any position under the latch could be observed. For the same reason, one side of the entrance box to the hood was covered with glass for observa-

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tion of the movements of rats before being admitted to the hood.

The underside of the latch was 2.5 cm. from the top of the table on which the problem box rested. This measurement was equal to the average height of rats 50 days old. This height was determined by experimentation, and was considered the most sat-

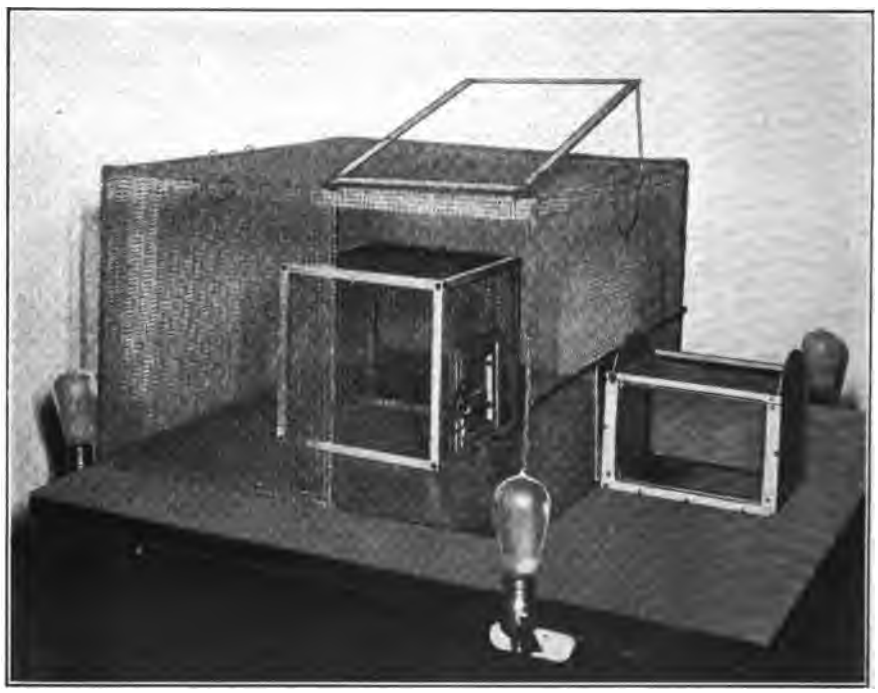

FIG. 1. LATCH-BOX PROBLEM

isfactory, in order to produce the most effective upward head-neck movement. When the latch was placed lower, the rat was found to assume an excessively crouching position; and when it was placed higher than 2.5 cm. the head-neck movement was often completed before the latch was raised out of the notch. The mechanism of the problem was thus adjusted to that of the reflex movement of the head.

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All the rats used in the experiment with the latch-box problem were 50 days old. The usual method of feeding for a day or two in the problem was thought essential. Sometimes with very timid rats a longer time was given. This period of feeding was regarded as the beginning of learning the environment of the problem and careful observation of the behavior of the rats was made at this time.

For a stimulus to activity, food was used. Because so many organic changes are dependent upon the taking of food and for nutrition, the food stimulus is no doubt the most satisfactory one to produce activity. Physiologically, it may be regarded the most convenient, and the most important one to produce the responses which the rat's organization is capable of making. According to Carlson (5, 6) augmented irritability sweeps over the nervous system during gastric hunger contractions of the alimentary canal. It must, as a consequence, put in a state of readiness, so to say, many reflexes which require only the external senses to produce progression and to solve the problem.

Obviously, the selection of food and its control are important considerations. Bread soaked in unskimmed milk was used. Such a mixture contains sufficient proteins, carbohydrates, and fats to maintain growth and activity. The physical condition of each rat was carefully watched in respect to the amount of food required for healthy metabolism. Usually the amount of food required could be discovered by exerting slight pressure on the abdomen of the rat before and after feeding. Pressure on the abdomen before feeding should not reveal a condition of relaxation of the abdominal muscles which indicates emptiness of the alimentary canal, and after feeding an exaggerated extended condition of the stomach and abdomen. Only sufficient food should be taken by a rat to maintain metabolism and growth. The exact amount needed by each rat can be determined only by experience.

Very early in experimentation the problem frequently remained unsolved after several hours time. So a given period of 30 minutes was established for each rat. If the problem at the end of this period was unsolved, the door of the problem box was opened

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and the rat permitted to feed within, and then placed in the living cage for a trial the next day. This method was continued for a total of 6 days. If the problem was then unsolved, the rat was considered unfit for solution of the problem and was no longer used. The one trial a day method was adopted throughout all investigations in learning.

Before the work had proceeded very far, it was clearly evident that the first trial was more important than succeeding trials for the consideration of the integrated movements in learning. During the supposed "initial effort" to solve the problem much of the support for a "trial and error" conception of learning either in its original formulation or in a modified form is obtained. At this time the rat is supposed to "to try" first one way then another until by the appearance of some fortunate "successful" movements "it solves" the problem. Diametrically opposite to the "successful" kind of movements, the "unsuccessful" are established. The latter are clearly the kind which do not produce the results for which we are looking and we regard those that do produce the results as the "successful" movements. The significant thing in this rating of movements is the regarding of "successful" movements as important, without consideration of the usual connection of events which lead up to the solution of the problem. In any scientific investigation, previously existing conditions have as great a value for the taking place of a process and its explanation as the last added.

When in preliminary feeding the rat is first admitted to the environment of the problem, his behavior is quite different from that when in the living cage. Usually in an active rat, numerous short spurts are made, and the body crouches at intervals and relaxes at the end of each spurt. With less active rats, progression is slow in all parts of the problem. At the termination of each spurt, the body is relaxed and progression is then in any direction. All these movements do not stand out alone, but are accompanied by evident organic changes, reflex excitability, and often with the voiding of excreta. Elliott (7) has shown that animals brought into strange surroundings had a considerably smaller amount of epinephrin in the adrenalin glands than other

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animals accustomed to an environment. If this small amount of epinephrin in glands is indicative of the existence of reflex excitability, the latter might be expected to accompany such behavior as noted in rats in a new environment. To say that the sensory impressions received and that the responses that follow, are "to familiarize the rat" with a new situation, is to express inadequately the significant changes that must take place in the organism. It is important to correlate these changes with those observed when a rat is for the second and third time admitted to the problem. Both the organic changes that can be said to take place and the gross movements performed have but one effect; they seem to fit the organism for any new change that may occur in the environment. Learning has begun with the first presentation of the problem. We have looked upon this period of preliminary feeding as one of preparation, and have not regarded it as having a cumulative effect on the organism to meet any additional change in the environment of the problem, such as shutting and fastening the door.

When the door of the latch box problem is closed, movements are more strikingly confined to the problem box, particularly to the door. In some rats activity is greatly augmented, but in others no perceptible increase can be detected. When activity is increased, it appears to be conditioned either, because the passage to the food through the door can not be made, or because the odor of food increases general activity. Progression then is here and there with the production of many common acts. These acts may be completely, partially, or incompletely produced. The common acts completely performed are those which are generally and effectivly performed, such as crouching of the body, climbing, and even at times playing. These movements are inherently conjoined or coordinated, not newly conjoined nor is any one of them modified to constitute a new or a complex movement. One kind of inherent fundamental movement passes to the other in a definite order. Climbing may be followed by running, progression may end in crouching or moving things about by an upward movement of the head, or pushing the snout into the wire mesh of the problem box or into the hood which covers

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the problem may be accompanied by crouching. All these movements that are completely performed are presented in a variety of groupings, and this indicates that these fundamental movements are somehow conjoined. They meet certain conditions of the environment of the cage, and they reappear in the new environment of the problem. Here manifold stimuli, or the configuration of the living cage, and that of the problem produce similar responses. The only difference in these movements in the two situations, that of the living cage and that of the problem, is that all movements in the problem are generally more quickly performed, But this is not always so, for in some rats movements may be performed slowly. A degree of reflex excitability often accompanies greater activity. On the other hand, at times fundamental movements are only partially or incompletely performed. These incomplete movements consist of reflex acts, such as ineffective scratching, biting, crouching, and pushing the snout into the wire mesh of the door of the problem. These movements are performed on hard resisting substances, and as a consequence are ineffectively and incompletely produced. Being ineffectively performed, these movements are usually accompanied by contraction of the extensor muscles of the entire body. Pronounced contractions of the extensor muscles produce posture for the performance of a definite act. Posturing is invariably seen in repeated crouching and setting of the body in the performance of an exaggerated movement. In the more active rats posturing is usually accompanied by a momentary increase in reflex excitability. This increase in reflex excitability indicates that widespread organic changes are taking place in the organism.

This division of all movements into complete and incomplete is of use for descriptive purposes only, and either of these terms used has no more fixed value than the other. The movements that are completely produced show that an effective adjustment to the outside of the problem has taken place, and those that are incompletely performed show an ineffective adjustment to the problem box. In many rats effective adjustment only to the exterior of the problem box inside the hood takes place. As a

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result, all the movements that do not lead to the solution of the problem can not be regarded as "unsuccessful," for the adjustment to, or the learning of, the environment outside of the problem box is as necessary as the adjustment to, or the solving of the problem. Again, the ineffective movements performed can not be regarded as belongng to an exclusive class, the "unsuccessful." for often no arbitrary distinction can be made between movements that are effectively and those that are ineffectively performed.

When all movements are closely examined, they show that the rat is already provided with a neuro-muscular mechanism for their production. The same thing seems to be true for the production of reflex excitability, and this is the reason why the term reflex is applied to the term excitability and not this last term alone used. All these manifestations are not to be regarded as instinctive or habitual in that they show a partial or a complete setting for a teleological end. No component of a movement is a definite integrated movement, disconnected or "random." If integrated movements are in any way disconnected, then the mechanism which produces them, one after the other, is disharmoniously connected together, but no evidence of this is observed in the structural relation of parts which produce these movements, and much less is this observed in the relation of one movement to the other. They are integrated reflex movements, conjoined, and similar to those evoked in a decerebrate or in a spinal animal, where one movement is recognized as an inherent part of the animal's organized mechanism. In such a mechanism, where one integrated reflex movement is inherently coördinated with another, there exists an interaction of these reflex parts with other organs and parts of the animal. In the behavior of the rat, this neural coördination of reflex movements, and this interaction of them with internal organs, is revealed in that connection of events of one response following the other, and not in a performance of separate integrated movements incoördinated, requiring sensory excitations to conjoin them. In the rat all movements performed are essentially coördinated with progression.

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In the majority of cases, it is the ineffective pushing of the snout into the mesh of the door just underneath the latch that eventually, with the crouching of the body, produces the neck reflex movement. Pushing is not effectively performed against hard resisting substances, and the body is immediately crouched and set. If at this time, the rat's muzzle or head comes at any instant in contact with the latch, an upward movement of the head and the body occurs. At times posturing of the body is not very pronounced when the latch is touched, but becomes so immediately on the contact of the rat's muzzle with the latch. In many cases, it is necessary for the latch to give away before posturing with the production of the neck reflex and the raising of the latch take place, just as the continuation of digging, or moving with the muzzle light substances is conditioned by these substances moving in order that the act be completed. Hard resisting substances produce ineffective movements.

All movements performed have a cumulative effect on the rat, increasing in many ways its efficiency. That such an effect from all activity is necessary for the production of the neck reflex movement, can be detected by observing closely the movements of individual rats. This effect is most evident in those rats in which the solution of the problem box comes early on the first trial. Two excursions around the problem box with the same number of tussles at the door, are quite sufficient to produce the neck reflex which raises the latch. All movements are quickly performed and reflex excitability persists almost to the point where solution occurs, then it subsides. With other rats, this cumulative effect, though not so observable, is nevertheless present. Besides the effective movements made at different parts of the problem box, ineffective movements are made repeatedly at the door before the latch is raised. Reflex excitability may be excessive, sporadic, or may not persist. When in rats excitability is excessive, considerable energy is used in raising the latch. Often it is thrown far out of the notch. Posturing under the latch is then excessive. Apparently many responses and organic changes are necessary to fit the organism for the production of the neck reflex, without which it is quite evident

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this movement could not take place. Again, some rats require the cumulative effect of one or more daily trials of 30 minutes each before the latch is raised. In such rats, movements are at the beginning of a trial or trials actively performed. Reflex excitability persists only a part of the 30 minutes and appears almost absent at the end of this period. For such animals, a greater time is required before certain responses are made, and organic changes occur, before the neck reflex really appears. And if these changes do not actively take place the problem remains unsolved. Previous responses and organic changes have little effect in eventually producing the neck reflex when the rat's muzzle touches the latch, or even when it is moved part way out of the notch. With such rats, it appears, reflex excitability with concomitant organic changes is to a great extent absent, and the changes that do take place do not fit the rat for the attainment of posture for the solution of the problem.

It can be determined experimentally that the cumulative effect of all movements with organic changes is a necessary condition for the production of the neck reflex movement, but this constitutes an additional problem for investigation. The effects that are produced can be shown and then judged by the number of times the latch is touched before the neck reflex is produced. The number of times the under surface of the latch was touched by the rat's head or muzzle was recorded on the smoked-drum of a kymograph. A light spring with electric contacts in circuit with a signal magnet was placed on the under surface of the latch. Records of 6 different litters are given in table 1. The positive sign in this table indicates that the muzzle of the rat touched the latch one or more times in immediate succession or at different times on that trial or day. The negative sign indicates no touches; the letter M, that the latch was moved part way out of the notch; and the letter S indicates the solution of the problem.

When a solution of the problem occurred, usually one or more touches of the muzzle were recorded before the neck reflex movement was produced to raise the latch. In some instances, though very rarely, the first touch of the latch resulted in the production

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TABLE 1

LITTER	RAT	DAYS					
		1	2	3	4	5	6
I	1	−	+	+	+	+	+
	2	−	−	−	−	−	−
	3	−	−	M	+	−	−
	4	+	+	+	−	+	−
	5	+	+	+	+	+	−
	6	+	+	+	S		
	7	−	−	+	+	−	−
	8	−	−	−	−	−	−
	9	+	+	+	+	+	+
II	1	−	−	S			
	2	S					
	3	+	+	−	+	+	−
	4	S					
	5	+	S				
	6	S					
III	1	−	+	+	S		
	2	+	+	+	+	−	−
	3	+	+	+	−	−	−
IV	1	S					
	2	S					
	3	+	+	−	−	−	−
	4	+	+	+	S		
V	1	+	+	−	−	−	−
	2	S					
	3	−	−	−	−	−	−
	4	+	+	S			
	5	S					
	6	+	+	S			
VI	1	−	−	+	−	−	−
	2	−	−	+	+	−	−
	3	−	−	+	−	−	−
	4	+	+	−	−	−	−
	5	+	−	−	−	−	−
	6	−	−	−	−	−	−
	7	−	−	−	−	−	−
	8	−	−	−	−	−	−

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of the neck reflex and the raising of the latch. As a rule, longer or shorter intervals between the first and the second touch and the production of the neck reflex occurred. The very first touch of the rat's muzzle usually appeared soon after the rat was admitted to the problem. When the problem remained unsolved, the under surface of the latch was either not touched, or it was touched several times and usually at long intervals. More often repeated touches occurred on the first 2 or 3 days, but not on the remaining 4 or 5 days. In some rats, inactivity followed short periods of activity on each of the 30-minute trials of the first days, and in other rats no evidence of activity was manifest at any time. With rat I, litter 2, the latch was not touched for 2 days, but on the third day, two touches resulted in the solution of the problem. In litter 6 solving the problem did not take place on any of the 6 days, yet touches were recorded.

The frequent failures to produce the neck reflex movement when the rat's muzzle touched the latch, are evidently due to the functional condition of the entire organism, and not to a dependency upon an adequate sensory excitation. For when the under surface of the latch is repeatedly touched and no adequate response is forthcoming, it appears that the neck reflex movement and associated reflex movements are undeveloped. Later it will be seen that certain fundamental, integrated, reflex movements are undeveloped and fluctuate. The important question is what is required to produce the neck reflex movement rather than the one of the reception of adequate stimuli to produce sensory excitations and the "successful" movement. What seems to be required is the initiation of an interaction of many body parts for posturing under the latch to produce the neck reflex movement. This interaction for posturing is less evident when reflex excitability is not manifest. Apparently an absence of these manifestations would indicate that some mechanisms are undeveloped, and consequently that interaction of body parts for the production of the neck reflex movement is dependent upon the functional development of many parts. If this is so, the interesting thing in the observation of an organism during learning, is the functional condition of the rat, and not what the rat does,

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even for some supposed definite reason, to produce the "successful" movement.

The functional condition of the rat can be determined only by close observation of movements that produce solution of the latch-box problem, and those that do not. Some data may be had from conditions that make the problem difficult to solve. It cannot be too strongly emphasized, that from observations of the ineffective movements produced, as well as of the movements that produce solution of the problem, a real understanding of the functional condition of the reflex mechanism required to solve the problem is to be had. Negative as well as positive results then, become for a time our chief concern, for we arrive at qualifications that are necessary both for the solving and for the learning of the latch-box problem.

To solve the latch-box problem, in addition to sensory excitations to produce the neck reflex, other reflex parts are necessary to produce posture, in order that an adequate neck reflex movement can be produced. The first evidence of posture is a crouching followed by a setting of the body. The setting of the body appears to be a momentary fixed position or a contraction of the extensor muscles of the entire body before the body is raised with the production of the neck reflex movement. With some rats, posture seems to be adequate when the neck reflex is readily performed, and then the crouching and setting of the body are not excessive. With some rats, posture may be quite adequate to produce the neck reflex movement, but may be excessive. In the vast majority of rats posturing is totally inadequate, and the neck reflex cannot be produced. When posture is excessive contracture of the body musculature is sometimes disproportionate in extent and complexity to produce the neck reflex, for the latch is sent out of the notch with great vigor when this movement with the raising of the body takes place. In some cases, before the neck reflex can be produced, successive attainments of postures are required, one succeeding the other at shorter or at longer intervals. Often it occurs that two neck reflex movements, or two pushes on the latch and two posturings of the body, one immediately followed by the other are made. In such cases

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the latch may be pushed only part way out of the notch and the neck reflex movement not completed. The body then is inadequately postured. Sometimes in excessive posture, the teeth of the rat are fixed in the wire mesh of the door under the latch and the body swayed back and forth and gradually raised. This upward movement eventually pushes the latch out of the notch, or at the moment excitations are produced from the moving latch, the hold on the wire mesh is released, and the neck reflex movement is produced. In such instances as the last, the stimulus of the moving latch becomes prepotent over touching the latch, and the neck reflex movement follows. Again, the teeth may be fixed in the latch and the latch raised out of the notch.

The appearance of posture to produce the neck reflex movement is dependent upon a degree of manifestation of reflex excitability. The most satisfactory performance of the neck reflex movement to raise the latch on the first trial and during learning follows a reflex adjustment of the body or posturing, and in some way the presence of reflex excitability is essential. Usually during and after posturing, an evident increase in excitability is manifest. Slight displacement of the latch not only increases posturing, but also increases reflex excitability which seems to aid the continuation of the performance of the neck reflex movement. Reflex excitability is again greatly increased in some rats after the neck reflex movement is produced. Rapid progression may occur in a direction away from the door, even around the problem. Feeding then does not commence immediately, but progression is continued in and out of the problem. Hunger excitations are evidently absent during manifestations of reflex excitability, and this in accord with the findings of Cannon (8) and Carlson (9) who have shown that in states of emotional excitement, peristaltic movements of the alimentary canal for the production of hunger excitations are inhibited. Only when reflex excitability subsides does feeding begin.

Hunger must also be absent or its excitations must be ineffective when inaction is present. Excitations as a result of contractions of the alimentary canal, can not for some reason be effective unless the mechanism of reflex excitability, and probably,

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other mechanisms are functionally developed. The degree of the efficiency of the organism depends upon the functional condition of the mechanism for the production of reflex excitability. In some rats reflex excitability is quite absent, and in some, it is manifest for only a short time. The duration of its existence is with most rats 5 or 10 minutes at the most, yet some rats show excitability a greater part of the 30 minutes given to solve the problem. Usually on each of the 6 days reflex excitability is manifest at the beginning of each trial and lasts a shorter time on each additional trial, or disappears completely after the third or the fourth day. At these times or on the sixth day, some rats very often appear less vigorous or are totally exhausted. Later in this paper, we shall have occasion to mention the appearance of exhaustion, and suggest conditions which probably determine it.

The foregoing descriptive results from observations of rats in the first trial are from 24 litters, a total of 156 rats. If reflex excitability determines the readiness with which the problem is solved on the first trial, other conditions in addition must determine the rapidity and the effectiveness of learning. The learning records of 14 litters, 80 rats, are presented in table 2. The results from 10 litters are not given for the problem was unsolved after 6 daily trials of 30 minutes each.

This table gives the number of individual rats in a litter, in how many instances the problem was solved, in how many it remained unsolved, and the day on which solution occurred. Learning was thought to be complete when for 15 times direct progression from the entrance box to the latch occurred, and the neck reflex was produced to raise the latch. Slight, imperfect responses made near the latch were not considered in this reckoning. This determination of the point when learning was thought complete was experimentally established when in the preliminary work with 10 rats or more, it was seen that 15 absolutely perfect records could not be obtained when from 60 to 125 trials were given.

Of the 80 rats recorded in table 2, solution of the problem occurred only in 40 per cent. On the first day solutions took place in 47 per cent of rats, on the second and third days in 19 per

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TABLE 2

NUMBER OF RATS	NUMBER IN LITTER	DAYS						NUMBER FAILED	NUMBER OF TRIALS
		1	2	3	4	5	6		
1	1	S							24
2	2	S							27
3	3	–	S					2	51
4	4	–	–	–	–	–	–		
5	5	–	–	–	–	–	–		
6	1	–	–	S					41
7	2	–	–	–	–	–	–		
8	3	–	S						45
9	4	–	S						38
10	5	–	–	–	S			2	38
11	6	–	–	–	–	–	–		
12	1	S							22
13	2	–	–	S					– Teeth
14	3	–	S					3	38
15	4	–	–	–	–	–	–		
16	5	–	–	–	–	–	–		
17	6	–	–	–	–	–	–		
18	1	S							19
19	2	–	–	–	–	–	–		
20	3	–	–	–	–	–	–	4	
21	4	–	–	–	–	–	–		
22	5	–	–	–	–	–	–		
23	1	S							29
24	2	–	–	–	–	–	–		
25	3	–	–	–	–	–	–		
26	4	–	–	–	–	–	–	3	
27	5	S							– Teeth
28	1	–	–	–	–	–	–		
29	2	S							32
30	3	S						2	35
31	4	–	–	S					45
32	5	–	–	–	–	–	–		
33	1	–	–	–	–	–	–		
34	2	–	S						46
35	3	–	–	–	–	–	–		
36	4	–	–	–	–	–	–	5	
37	5	–	–	–	–	–	–		
38	6	–	S						42
39	7	–	–	S					54
40	8	–	–	–	–	–	–		
41	1	–	–	–	–	–	–		
42	2	–	–	–	–	–	–	4	
43	3	–	–	–	–	–	–		

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TABLE 2—*Continued*

NUMBER OF RATS	NUMBER IN LITTER	DAYS						NUMBER FAILED	NUMBER OF TRIALS
		1	2	3	4	5	6		
44	4	–	–	–	–	–	–		
45	5	–	–	–	–	S			126 Failed
46	1	–	–	–	–	–	–		
47	2	–	–	–	–	–	–		
48	3	–	–	–	–	–	–		
49	4	–	–	–	–	–	–		
50	5	–	–	–	–	–	–		
51	6	–	–	–	S			8	46
52	7	–	–	–	–	–	–		
53	8	–	–	–	–	–	–		
54	9	–	–	–	–	–	–		
55	1	–	–	–	S				47
56	2	S							34
57	3	–	–	–	–	–	–	1	
58	4	S							32
59	5	–	–	S					55
60	6	S							34
61	1	S							31
62	2	S							34
63	3	–	–	–	–	–	–		
64	4	–	–	S				1	38
65	1	–	–	–	–	–	–		
66	2	–	–	–	–	–	–		
67	3	–	–	–	–	–	–		
68	4	–	–	–	–	–	–	5	
69	5	–	–	–	–	–	–		
70	6	–	–	–	–	–	S		56
71	1	S							34
72	2	–	–	–	–	–	–		
73	3	–	–	–	–	–	–	4	
74	4	–	–	–	–	–	–		
75	5	–	–	–	–	–	–		
76	1	S							19
77	2	–	–	–	–	–	–		
78	3	–	–	–	–	–	–	4	
79	4	–	–	–	–	–	–		
80	5	–	–	–	–	–	–		
80	14	15	6	6	3	1	1	48	

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cent, on the fourth day in 9.3 per cent, and on the fifth and sixth days in 3 per cent of rats. This per cent estimate includes cases in which the latch was raised by the teeth instead of by the neck reflex. Only in 3 per cent of rats were the teeth used. Learning was possible in nearly all cases when the neck reflex movement was used, with the exception of a solution occurring on the fifth day. Though learning was possible when solution in one rat occurred on the sixth day, many slight imperfect responses occurred before the latch, which were not considered in the general estimate when learning was complete. From the first to the sixth day, there was a decrease in the per cent of solutions, and with the rats that solved the problem between the first and the sixth day, there was generally an increase in the number of trials to complete learning.

The descriptive account of the observations made on rats and the results presented in table 2 do not determine what conditions both the observations and the results obtained. However, something does seem conclusive from the observations of the behavior of rats, namely, that reflex excitability is an essential qualification in establishing an interaction of body parts for posturing under the latch to produce the neck reflex movement; and when posturing can not be produced, the mechanism for the production of reflex excitability appears to be undeveloped. But this does not mean that posturing of the body can not at any time take place when this manifestation is not in evidence, for ineffective posturing may occur with no evidence of reflex excitability manifesting itself before or after posturing. The effectiveness of posturing and every response is markedly increased with the presence of excitability. Another condition, however, determines the rapidity of learning, and this is the number of imperfect responses made. There exists the greatest difference in rats in the number of such responses made. When learning is completed in 19 to 25 trials, few if any imperfect responses are made. From the third or the fourth trial progression is then direct to the latch and the neck reflex to raise the latch is easily produced. When in one rat 19 trials were required to complete learning, direct progression to the latch and

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posturing to raise the latch occurred on the fourth trial, and not a single imperfect response was recorded. The superiority of this rat was such that an adequate explanation can not at this time be given.

One thing, however, was clearly noticeable. When in one rat of a litter marked efficiency in learning the latch-box existed, other members of the litter showed efficiency in this direction, or the behavior of a member of the litter revealed evidence that the problem might be solved; and when the solving of the problem did not take place in any member of a litter, the behavior of only one or two members showed that the problem might be solved. There is a high correlation between members of a litter when the problem is effectively solved, and a low one when the solving of the problem was impossible. An explanation of this high and this low correlation in litters can not be attributed to a difference of "intelligence" in rats, for no such factor can be supposed to condition ineffective posture under the latch, and the production of imperfect responses. Moreover, if "intelligence" is indicated by the efficiency of the performance of movements, then when the problem remained unsolved at the end of 6 days, a low limit of "intelligence" or its entire absence must be assumed. There is danger if such a norm as "intelligence" is accepted, that this will become our chief concern, and not the conditions that actually determine and do not determine the solution of the problem.

Some additional observations of the movements of rats led to the belief that there exist in many rats functionally undeveloped reflex mechanisms, and this led to additional experimentation and tests of rats. Some idea of undeveloped integrated reflexes was obtained when it was observed that posturing to produce the neck reflex was difficult or impossible, and as a consequence imperfect responses were produced. Observations of the behavior of rats in the problem did not determine for a certainty that the reflex mechanisms were undeveloped, for the neck reflex movement was produced rapidly, too rapidly in fact, for good observation.

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The only means to determine the undeveloped reflex mechanism was to test the living rat for the neck reflex movement and its interaction with other reflex mechanisms. Three different methods to obtain desired results were tried. The first was to tap the rat's snout gently with the forefinger of the right hand to test the threshold for touch; the second was to press the snout ventralward and immediately release the pressure. This last method was as close a reproduction of conditions which produce the neck reflex to raise the latch as could be devised. Practically, the same results were obtained from both tests. There was either a slight stiffening of the neck muscles, or a slight raising of the head dorsally, and in some cases to the midline of the body before stiffening of the neck muscles occurred. Movement of the head was less general than were contractions of the muscles of the neck which resulted in the stiffening of the neck. Very little coördination of the reflex movement of the neck with the other body integrated reflexes was evoked; but rats that gave any indication of an interaction of the neck reflex with other body parts showed, when in the environment of the problem, evidence of reflex excitability.

The third test was productive of better results; the rat's head was passively raised dorsally by placing the forefinger under the lower jaw to a position approximately at a right angle with the midline of the body. This test in many rats immediately evoked remarkable reflexes. The number of them varied greatly in rats and in some there was produced a coördination with other groups of reflexes which were regarded as necessary for the attainment of posture under the latch. Once more this coördination was much more manifest in rats that showed a degree of reflex excitability. When in rats interaction of other body parts occurred with the production of integrated coördinated reflexes, a fair prediction was often made that posturing with the raising of the latch of the latch-box problem would take place, and in every case when the prediction was not fulfilled, reflex excitability soon subsided.

Just what the nature of these reflex disturbances was could not at once be made out. They appeared to be native complex re-

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flexes associated with the movements of the head. A review of the literature on the physiology of reflexes showed that within recent years there have been conducted a number of investigations of the reflexes evoked in decerebrate animals with a change in the position of the head in space. Very few experiments have been made with living animals. Magnus and de Kleijn (10, 11) working with decerebrate rabbits, cats, and dogs have isolated definite reflexes. With passively changing the position of the head in relation to the body, continuous excitations in the labyrinth of the ear evoke movements of all four limbs. A change in the position of the labyrinth in space and the position of the neck in regard to the body, increases in decerebrate rigidity, the tonic condition of the limbs.

When the head is raised, the lower jaw forming an angle of 45 degrees with the midline of the body, the extensor tone of the fore limbs increases, and that of the hind limbs decreases at times into flexor tone. When the head is lowered to within 45 degrees of the midline of the body, the reverse conditions are produced, the extensor tone of the hind limbs increases and that of the fore limbs decreases, sometimes into flexor tone. On the other hand there arises, when the head is bent to the left or to the right, or when the head is twisted in either of these directions, an increase extensor tone of the fore limbs on the side from which the head is bent and a decrease in tone, often amounting to flexion, on the side to which it is bent.

Magnus and de Kleijn call the first of these reflexes, those derived by changing the position of the head in space, the labyrinth reflexes, and the other two, derived by bending of the neck in regard to the position of the body, neck reflexes. Sherrington (12), who has also investigated these reflexes, regards such manifestations of increase extensor tone as modifications of reflex posture of standing decerebrate rigidity. For when the head of the standing decerebrate preparation is raised dorsally, the postural contractions of the fore limbs increase and the fore quarters are raised, whereas the postural contractions of the hind limbs are inhibited, flexed, and the hind quarters lowered. The reverse is the case when the head is passively flexed; the postural con-

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tractions of the fore limbs are inhibited, the fore quarters are lowered and those of the hind quarters raised. This last effect is also obtained when the tail is dorsally raised. Modifications obtained by bending the head and neck to the right, producing increased extensor tone in the left fore limb is similar to what normally occurs when the animal in standing posture, gazes to the right; the reverse is the case when the head is bent to the left, extensor tone of the right limb increases when gazing to the left.

From these investigations, particularly those of Sherrington, some of the reflexes revealed may be said to be used when the rat solves the latch-box problem. Additional observations with other rats substantiated this view. The neck movement to raise the latch is accompanied by increase extensor tone of the fore, and flexion of the hind limbs as had previously been observed. For when this movement occurs in rats, the fore limbs are seen to be extended and the hind flexed, and this reflex posture is identical with that obtained from a decerebrate cat when the head is raised dorsally. Concerning any integration of these reflexes with the trunk muscles nothing has been said; but Sherrington has shown that the extensor tone of the trunk muscles increases with the bending of the trunk muscles upwards, not downwards. Here there is something parallel with what appears when the neck reflex is produced to raise the latch, for at that time the trunk is raised with the movement of the head upwards. The integration of the trunk muscles, though not clearly observed in the rat when the head is dorsally extended, is evidently present when the act of raising the latch is complete, for then the body of the rat is reflexly held momentarily in a rigid posture before progression begins to the food. Reflex extensor tone in the limbs or the trunk muscles must also exist when, in posturing, the body is set prior to the production of the neck reflex. According to Sherrington reflex extensor tone exists in those muscles that are anti-gravity, and setting of the body at this time, apparently produces an increase in reflex extensor tone in those muscles that prevent excessive crouching, but permit a setting of the body, sufficient to raise the latch. Evidently there is reflex inhibition or relaxation of some muscles, and an increase tonicity in those

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muscles that would at all times maintain the animal in a position for solving the problem. These last muscles embrace those of the limbs, trunk, neck and head that aid the posturing of the body for the production of the neck reflex to raise the latch.

If our description of the behavior of the rat is a correct one, the increase reflex-extensor tone in anti-gravity muscles must necessarily be more manifest in those rats that show reflex excitability. Beritoff (13), who has also investigated the increase extensor tone in all limbs with the changes of the position of the head and tail in space, states that reflex extensor tone is more readily obtained in reflex excitable specimens than in those specimens that do not manifest reflex excitability. Such experiments as have already been performed with rats have shown that a greater interaction of the body parts is present at all times in those forms that were reflexly excitable. Moreover, it appears that reflex excitability is given expression by increase extensor tone of muscles rather than in flexion of the muscles.

To determine to what extent the position of the labyrinth of the ear, or the head in space, and the extending of the tail dorsally, increased the extensor tone of the limbs, or resulted in modification of reflex posture in rats, over 300 rats were tested and examined for the extensor tone of the limbs and their interaction with the reflexes of the trunk. Any manifestation of reflex excitability was noted when making these tests. Our aim here is to confirm such points as were derived from direct observation of the behavior of rats in the problem, and to determine if possible any variation in the mechanism of reflexes, and in the production of reflex excitability, which would effect learning. The test of the rat's reflexes were made before each trial and, occasionally, after the interval of thirty minutes when the latch-box remained unsolved.

Before the method of the examination of reflexes was perfected, it became evident that the reflexes evoked in the living rat could not be regarded as producing alone increase extensor tone of the limbs in decerebrate rigidity, but revealed variations in extensor tone of rhythmic extension and flexion of the limbs or a single

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thrust[1] of a limb. A single thrust was often evoked from an ipsilateral fore thrust, but more often from both hind limbs. Great variation in the production of a series of thrusts or of a single thrust exists.

When the method of examination of the thrusts was perfected, the rat was held in the left hand in a position which permitted free movement of the limbs. To evoke the extensor thrust of the fore limbs, the rat's head was passively extended dorsally until the lower jaw was to the midline of the body or within 45 degrees of this line, and also bent to the right and to the left of the midline. To evoke the extensor thrusts of the fore limbs when the head was dorsally extended, the right fore finger was placed under the lower jaw and the head gently but firmly raised; to evoke the extensor thrusts with bending the head, the muzzle of the rat was seized and gently moved to the right or to the left, to a point where rhythmic extention and flexion of the limbs were less pronounced. The extensor thrusts of the hind limbs were evoked by dorsally extending the tail and slightly raising the hind quarters of the rat.

The rat being a very docile creature when tame, no great difficulty was met in examining the extensor thrusts of all limbs. When a rat showed excessive reflex excitability, slight trouble was experienced in holding the animal in the right position to evoke the extensor thrusts; the animal struggled to get away. In such instances, an examination of these reflex extensor thrusts on the following day gave better results. Reflexes were evoked whether the head and tail were passively moved slowly or quickly. This indifference in regard to the rate of movement of the head to evoke the extensor thrusts is probably due to the fact that changes in the position of the head arouse continuous excitations in the receptors of neck muscles and in the labyrinth of the ear. Passively extending the tail dorsally produced excitations in the receptors of the tail and hind quarters. The effects, however,

[1] The term extensor thrust has in physiology been applied only to the extension reflex elicited on stimulation of the foot of a spinal animal. Proc. Roy. Soc., lxvi, p. 67, 1900. The use of this term in a wider sense, it is hoped will not be confusing.

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are not so often localized as this description may indicate, for the integrated reflexes of any animal are coördinated.

When the functional condition of the reflex extensor thrusts can be said to be the best, and reflex excitability manifest, passively bending the head and the neck to the right and to the left or extending the head dorsally to the midline of the body or within 45 degrees of this line, evokes extension and flexion of the fore limbs with concomitant changes in the hind limbs and the tail. Passively bending the head to the right evokes an extensor thrust of the left ipsilateral fore limb and flexion of the right contralateral fore limb extension of the right contralateral hind limb and flexion of the left ipsilateral hind limb; the reverse of these conditions is obtained when the head is passively bent to the left, for then extension of the right ipsilateral fore thrust with flexion of the left and extension of the left contralateral hind limb with flexion of the right ipsilateral hind limb occurs. The tail is raised dorsally and frequently inclined in the direction in which the head is bent; dorsally extending the head evokes extension of both fore limbs and flexion of the hind limbs with contractions and extension of the trunk muscles. Extending the tail dorsally evokes extensor thrusts of both hind limbs and flexion of the fore limbs. Thus as previously stated rhytlmic extension and flexion may occur in all limbs, instead of the evoking of independent extensor thrusts.

These extensor thrusts in the rat's limbs are in a way practically the same as the increase extensor tone observed to take place in decerebrate cats and dogs, for they are conditioned by similar excitations and similar antagonistic muscles of the limbs are involved. In the living animal the increase tone is revealed in extension and flexion of the limbs, and in decerebrate forms in the increase tone of decerebrate rigidity. The direction of extension of the limbs is in the two cases also different. The increase tone in the decerebrate animals extends the limbs directly downward or outward in accordance with the position in which the animal is placed, whether resting on the back or in a standing posture; in the living rat, the direction of the fore extensor thrust when head is moved is in the direction in which the head is passively

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moved. The hind limb thrusts, when the tail is dorsally extended, are directed downward and backward.

The exact nature of these reflex extensor thrusts must be studied in their harmonious coöperation with other reflexes and their importance in the behavior of rats for all integrated reflexes are coördinated. In whatever position the head of a well developed rat is passively moved, reflex extension of some limbs and reflex flexion of others occurs, with contraction and relaxation of the trunk and back muscles. If the head of the rat is passively bent to the right of the midline of the body, then to the left of this line, extension and flexion of all limbs takes place in the manner of rhythmic progression. The direction of these thrusts is toward the side in which the head is bent. These extensions and flexions of the limbs reveal conditions for progression to one side, or around an object and not directly ahead. These thrusts in addition to revealing posture with the head turned to one side, as Sherrington maintains, reveal the manner of progression in the direction in which the head is turned. Furthermore the reflex thrusts evoked when the head is dorsally extended to 45 degrees of the midline of the body, show not alone, as Sherrington again states, a modification of reflex standing posture, the animal assuming a posture in the act of looking up at an object with the fore limbs extended and the hind flexed, but also in the act of jumping, or moving an object upward with the muzzle. The same thing is true of the condition of the limbs when the tail is dorsally extended or when the head is bent ventralward. This position of the body is not exclusively for looking under objects, but also for moving low objects about with the muzzle and for progression under objects. In decerebrate forms, only one condition is revealed—a modification of the reflex standing position; in the living animals a modification of progression as well is noted, such as jumping, crawling under objects, and moving objects about with the muzzle. In many cases there results not only a modification of a standing upright position, but also progression in an upright position. When objects are raised upward by the muzzle of an animal, there exists either a modification of the reflex standing position or a modification of an up-

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right position during progression, in accordance with the existence at the time of a reflex standing position or of progression, before there occurred a modification of the position of the limbs and body of the animal. Both modifications, that of a standing position before the latch of the latch-box problem, and one of upright progression to it, may occur in rats when standing, or when progression existed before posture was attained to produce the neck reflex movement. Evidently, all movements are inherently coordinated with progression, for rhythmic extension and flexion of the rat's limbs are more fundamental than any other integrated movements produced in the rat.

Great variation exists in the functional condition of all reflex thrusts of both fore and hind limbs. When their functional condition is the best, passively moving the head slightly, or even touching the snout or vibrissae will at once evoke strong extensor thrusts. Opening the mouth or tickling the lips will produce similar responses of the limbs, with shaking of the head. On the other hand when these reflex thrusts are poorly developed, they can sometimes be evoked on the first and succeeding tests, the extensor tone gradually growing stronger with an increase spread of excitations in the receptors of the neck and the labyrinth of the ear, or the extensor tone becomes weaker with each test, or, the thrusts can be evoked on the first test and not thereafter. Sometimes the reflex thrusts cannot be evoked on the first test, but only on the second, third or fourth test, and then the extensor tones are usually weak. Those evoked on the second test have usually a stronger extensor tone than those evoked on the third or fourth. In some rats no reflex extensor thrusts can be evoked after repeated tests. The individual differences are very great in respect to the time of the appearance of these reflex thrusts, and this seems to indicate an unequal antagonistic action of extension and flexion of them or an undeveloped condition of them. This undeveloped condition must reside in the centers for the production of these thrusts, for repeated tests do not in all cases spread excitations in receptors to evoke them. An increase in excitations happens only when repeated tests increase the extensor tone of the limbs.

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When the head is passively bent to the right or to the left of the midline of the body, either only one of the two ipsilateral fore thrusts, or the one with a stronger extensor tone than the other, is evoked. More often, in either case, a right ipsilateral fore thrust prevails. At times a contralateral right or a left fore thrust is synchronously evoked with an ipsilateral fore thrust; this seldom occurs when a left ipsilateral fore thrust is evoked, but is rather common in the case of a right ipsilateral fore thrust. Again the right ipsilateral fore thrust is more often present than the left, when the rat's head is dorsally extended 45 degrees of the midline of the body. The functional condition of the reflex thrusts of the hind limbs, when the position of the head is changed in space, follows closely the development of the fore limb thrusts. Usually when an ipsilateral fore thrust has a strong extensor tone or one has a stronger extensor tone than the other, its contralateral hind has a strong extensor tone, and when the ipsilateral fore has a weak tone, its contralateral has a weak extensor tone or lacks it altogether. When the tail is dorsally extended, both hind limbs may be rigidly held backward, or they may be rhythmically repeated alternately. At times, one ipsilateral hind thrust is evoked with a stronger extensor tone than the other, or both extensor thrusts may be absent. In some cases the reflex extensor thrusts of all limbs cannot be evoked.

A few words will not be amiss in stating what is meant when it is said that an ipsilateral fore thrust has a strong or a weak extensor tone or that it is absent. In this paper when it is stated that extension of a limb is not followed by flexion of its contralateral, or extension is weak, or absent, it is meant that the reflex extensor thrust or its mechanism is undeveloped. This statement is however, not quite justifiable when a strong extensor tone in an ipsilateral thrust of the fore limbs is evoked and when flexion of its contralateral is not at the same time evoked, or when only a strong extensor tone in one ipsilateral fore extensor thrust can be evoked and not its contralateral. There appears here to be a distinct difference between the evoking synchronously of extension and flexion of the fore limbs. When an ipsilateral reflex extension of one fore limb is evoked with flexion

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at the same time of its contralateral, or when the ipsilateral extensor thrust of the other side of the animal can not be evoked, there being only one predominant reflex thrust present, then this might indicate the existence of a unitary reflex rather than a diphasic system of antagonistic action of flexion and extension of the limbs. A diphasic system is present when there is revealed symmetrical development, and a unitary mechanism when the fore reflex thrusts are asymmetrically developed. A unitary assymetrical development with a strong extensor tone in a right or a left ipsilateral thrust must, with extensor tone in a weak fore reflex thrust, be regarded for the present as an undeveloped condition of the reflex extensor thrusts. This statement is made with some reservation, for the right fore ipsilateral extensor thrust is more often evoked than the left, and it will be shown later that the right side of the animal invariably predominates over the left. The great predominance of the right fore thrust over the left may indicate that the tendency to righthandedness exists or is developing in the rat.

The reflex extensor tone of a limb may be considered weak when a thrust is either incompletely, or completely but feebly, evoked. Little need be said about weak extensor tone in reflex extensor thrusts when these thrusts are completely evoked, other than that the reflex thrust is slowly and not vigorously produced. When they are incompletely evoked, the extensor thrust may involve only the shoulder, the elbow, or the extension of the paws of the fore limbs, or the knee of the hind limbs. Sometimes flexion of the limbs prevails instead of extension. This is often the case when it is said that all reflex thrusts are absent or can not be evoked. Flexion may then be regarded as action, as well as extension, but of an opposite kind. All transitional stages exist, and such stages may be regarded as revealing the existence of a marked undeveloped functional condition, or an unequal antagonistic action of extension and flexion of a rat's limbs.

In addition to the existence of a functionally undeveloped condition of the reflex extensor thrust, instability of them is exceedingly common. Fluctuations in the reflex thrusts occur invariably when one extensor thrust has a stronger extensor tone than

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another, or when an ipsilateral fore reflex thrust can alone be evoked. These fluctuations occur after repeated tests, or at greater intervals from day to day, and consist in an increase or in a decrease in the extensor tone of the limbs, at times amounting to a total disappearance of one or more of the reflex thrusts, or to a reappearance of all thrusts when one or more are absent. In some rats the reflex thrusts may not fluctuate for several days, all remaining vigorous and strong, when suddenly they fluctuate. Only in a few rats is the stability of the reflex thrusts fairly constant, either after immediate successive tests at one time, or after daily tests. Of all the rats tested, only 2 of the 300 or more had developed reflex thrusts, and fluctuation of them did not take place to any great extent. Slight decreases in extensor tone were alone noticed and then at the end of learning. Fluctuations of the reflex extensor thrusts, then, are the general rule in rats and may be said to be more evident in the fore limb thrusts than in the hind limb thrusts.

What conditions this fluctuation of the reflex thrusts cannot be considered at this point, but a few remarks will have some bearing on observations to be noted later. There are indications that environmental influences condition in a measure these fluctuations. In the daily investigation of them, giving only one test for each limb, it was early observed that marked changes in the environment seemed to increase not only the degree of, but also the frequency of the fluctuations. Removing the rat from the living cage to the problem often produced in many rats rapid and marked fluctuations. Sometimes the extensor tone of the limbs was particularly strong when a rat was first handled or when put for the first time in the problem, and sometimes gradually, sometimes suddenly, when the new situation was about learned, a decrease occurred in the extensor tone and some reflex thrusts disappeared. This fluctuation may be due to the fact that in a new environment reflex excitability, which is aroused because of a change, increases the tonic reflexes of the body including those of the limbs. Decided changes in temperature, such as a rapid rise or fall, have occasionally been noted to produce fluctuations in the extensor thrusts. Decrease in the extensor

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tone of the extensor thrusts frequently occurred with a rise in temperature and in humidity, and a slight but sudden fall in temperature during the summer months after a hot spell had the opposite effect, increasing the extensor tone of the limbs. This increase in extensor tone might be expected to take place with a lowering of temperature, for there is, as measured by the output of carbon dioxide, and the intake of oxygen, a higher metabolic rate at lower than at higher temperatures. Reflex stimulation of the skin by reduced temperatures, in all probability, increases this metabolic rate and the tonic reflexes are benefited thereby. The reverse would be the case when the temperature is increased, tonic reflexes would be diminished in strength, inclusive of the extensor thrusts of the limbs. Sometimes when fluctuations occurred in one member of a litter, they took place in other members of the litter. Members of other litters revealed but the customary fluctuation in reflex thrusts. Possibly some litters are more easily affected by changes in the environment than others. At times it appears that fluctuation in certain limbs is characteristic of one litter and not of others. At present it is difficult to state what may condition fluctuations in one litter and not in another. Sometimes the taking of food increases or decreases the extensor tone of the thrusts. In some rats a variety of conditions produces fluctuations in these thrusts. These fluctuations will presently be shown to condition variations in the behavior of rats or in the methods of learning problems.

Obviously, there exists one satisfactory evidence of Jennings' (14) statement that there occurs, as a result of changes in the environment, a change in the physiological state of an organism; however, this state must not be regarded as something permanent and static, but a change in the functional condition of the organism. Soon it will be evident that learning is not exactly a ready resolution of the physiological state of an organism, but is to be regarded as the facilitation, or, the more ready working of an exaggerated performance of coördinated, integrated reflexes interacting with many other parts of the body. These cöordinated reflexes which solve the problem persist, because a resolution takes place, which is essentially the development of

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a greater interaction of body parts. Limitations to learning are dependent upon this development of a greater interaction of body parts.

Before proceeding with the results obtained from the investigation of the reflex mechanism of extension and flexion of the rat's limbs and its connection with the learning of the latch-box problem, it is well to consider the mechanism for the production of reflex excitability. The assumption from early observations of rats that reflex excitability was the product of a mechanism of some kind, is in line with the statement of Beritoff (15), that in decerebrate animals, reflex excitability is often manifest, and that in such preparations the extensor tone of the limbs is the best. It seems true from another point of view that the mechanism for its production interacts with that of body reflexes, inclusive of extension and flexion of the limbs, in that in rats in which excitability is manifest, the solving of the problem is certain. One unfortunate thing, is that the degree of the development of the mechanism for the production of reflex excitability is difficult to judge. This is particularly unfortunate since the degree of the development of the extensor thrusts can be judged, not through observations of the behavior of the rats in the problem as is the case with reflex excitability, but through actual tests on the rats. The only evidence of the degree of the development of reflex excitability is to be recognized in the appearance of rapid, slow, or constant activity, or in the absence of activity. Realization of the mechanism of reflex excitability is accordingly, far from being as tangible as the realization of the mechanism of the extensor thrusts. Degrees of reflex excitability manifest in rats may be conveniently designated as hypernormal or hyponormal.

When the reflex thrusts are symmetrically developed and reflex excitability is hypernormal, the latch box is soon solved. Early solving of the problem comes as a result of the fundamental integrated reflex movement being effectively performed, and a *number* of ineffective movements are not made. Usually progression occurs around the problem 3 or 4 times with 1 or 2 posturings and touches of the under surface of the latch and the

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neck reflex is produced. Not one fundamental reflex movement is difficult to perform. Posture is not excessive; that is, a large number of muscles of the body do not contract to produce it, and the neck reflex movement is very readily produced. Rapid adjustment to the environment of every part of the problem takes place, and the movements made, seem effective for every part of the latch-box problem. The cumulative effect of all movements performed soon establishes interaction of body parts for the production of the neck reflex.

This account holds good when the reflex mechanisms of the rat are functionally developed, but if timidity is manifest, the problem is not solved until this manifestation disappears, and should timidity persist, the problem remains unsolved. Sometimes all the reflex thrusts may be evoked on the first trial and for a few succeeding trials they appear functionally well developed, and then fluctuate Solution of the problem, then may not occur so soon on the first day or not until a day or so later, at times not until the reflex thrusts have again fluctuated. Evidently in such cases, instability of the reflex thrusts or the possibility of instability in the future determines or prolongs the time when the problem will be solved.

This last statement would also apply to rats in which the reflex thrusts on the first trial are undeveloped. But here conditions exist, depending upon whether reflex excitability is hypernormal, or hyponormal. When reflex excitability is hypernormal and the reflex extensor thrusts are functionally undeveloped or asymmetrically so, then solution of the problem comes very soon, sometimes as soon as when the reflex mechanism of the thrusts is fully developed. Active movements result in posturing and the production of the neck reflex; but posture is usually excessive and a large number of muscles are used for its production. The latch is raised by the neck reflex with great force and suddenness. When reflex excitability is hyponormal, the solution of the problem does not take place usually on the first day, but may occur on any one of the suceeding 5 days. The mechanism for the production of reflex excitability must either be developed, or an interaction of the body parts of the animal is slower in appearing

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than when excitability is hypernormal. Often, as previously stated, when reflex excitability is hyponormal, the problem remains after 6 days unsolved. In some cases in rats in which at the beginning of a trial excitability is hypernormal, it later subsides and becomes hyponormal. It is once more evident that the solution of the problem is dependent upon a manifestation of reflex excitability in rats, but the efficient manner in which the solution of the problem occurs, is dependent upon the functional condition of the reflex thrusts of the rat's limbs. The inefficient ways previously observed to have occurred when solving the problem, may now be accounted for by the undeveloped condition of the reflex extensor thrusts. One way, the use of the teeth to raise the latch out of the notch, is of particular interest. A different method to solve the problem comes about as a result of the functional undeveloped condition of the extensor thrusts and not because of repeated trials or successive "efforts." The use of the teeth is the direct result of the ineffective production of posture to produce the neck reflex; yet posture of a kind to raise the latch with the teeth is observable. Both the production of this posture and the use of the teeth are modifications of posture and the production of the neck reflex movement, for when the mouth is opened the fore limbs are extended and the hind flexed in the manner employed with the production of the neck reflex. In both instances the fore quarters are raised and the hind flexed. This reflex phenomenon is more evident when the jaw is lowered with the opening of the mouth than when the head is dorsally extended. It has been noticed that when the teeth have been used for a time, and a decided fluctuation in the reflex thrusts occurs, the neck reflex may be used instead of the teeth to raise the latch or vice versa.

These factors that determine the solution of the latch-box problem are presented in table 3. The effectiveness or ineffectiveness with which the latch was raised out of the notch to open the door of the problem is not indicated in this table, but this will be given in other tables in connection with learning. In the present table are indicated the develoment of the reflex thrusts, their stability or instability, and the degree of reflex excitability

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TABLE 3

	REFLEX THRUSTS				REFLEX EXCITABILITY	DAY SOLVED	
	Fore	45 degrees	Hind				
1	Both	Right	Both	Fluctuated	Hyponormal		Unsolved
2	None	None	Both	Fluctuated	Hyponormal		Unsolved
3	Right	Right	Both	Fluctuated	Hyponormal		Unsolved
4	L. str.	None	Both	Fluctuated	Hyponormal		Unsolved
5	Both	None	Both	Fluctuated	Hyponormal		Unsolved
6	R. str.	None	Both	Fluctuated	Hypernormal for a time		Unsolved
7	Both	None	Both	Fluctuated	Hypernormal for a time		Unsolved
8	R. str.	None	Both	Fluctuated	Hypernormal for a time		Unsolved
9	R. str.	None	Both	Fluctuated	Hyponormal		Unsolved
10	R. str.	Right	Both	Fluctuated	Hyponormal		Unsolved
11	Both	None	Both	Fluctuated	Hypernormal for a time		Unsolved
12	Both	None	Both	Fluctuated	Hyponormal		Unsolved
13	B. wk.	None	Both	Fluctuated	Hyponormal		Unsolved
14	Both	R. str.	Both	Fluctuated	Hyponormal		Unsolved
15	R. str.	B. wk.	B. wk.	Fluctuated	Hyponormal		Unsolved
16	Both	Right	Both	Fluctuated	Hypernormal	1st	
17	Both	Right	Both	Fluctuated	Hypernormal for a time		Unsolved
18	Both	None	Both	Fluctuated	Hypernormal	1st	
19	Both	R. wk.	Both	Fluctuated	Hypernormal	1st	
20	Both	Both	Both	Fluctuated	Hypernormal for a time		Unsolved
21	Both	Right	Both	Fluctuated	Hypernormal	1st	
22	Both	None	Both	Fluctuated	Hypernormal	1st	
23	None	None	Both	Fluctuated	Hypernormal	2d	
24	Both	Both	Both	Fluctuated	Hypernormal	1st	
25	L. wk.	None	Both	Fluctuated	Hypernormal	2d	
26	Both	None	Both	Fluctuated	Hyponormal		Unsolved
27	Both	Both	Both	Fluctuated	Hypernormal	1st	
28	R. wk.	None	Both	Fluctuated	Hypernormal	4th	
29	Right	None	Both	Fluctuated	Hypernormal	4th	
30	Both	B. wk.	Both	Fluctuated	Hypernormal	1st	
31	Left	None	Both	Fluctuated	Hypernormal	1st	
32	Both	Both	Both	Slight	Hypernormal	1st	
33	B. wk.	Both	Both	Fluctuated	Hypernormal for a time		Unsolved
34	Both	Right	Both	Fluctuated	Hypernormal	1st	
35	B. wk.	B. wk.	Both	Fluctuated	Hypernormal	1st*	
36	Both	None	Both	Fluctuated	Hypernormal	1st*	

* Teeth used.

B. or L. wk. = both or left weak; R. or L. str. = right or left stronger.

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manifest by rats. It will be noticed that only in one rat were the reflex thrusts functionally developed, and in all other rats these thrusts were undeveloped. Reflex excitability is recorded as hypernormal, or hyponormal. When reflex excitability was hyponormal the solution of the problem occurred from the second to sixth day or it did not take place. Two rats, in which undeveloped reflex thrusts existed used the teeth to raise the latch instead of the neck reflex.

Observations of the movements of the rats presented on this table indicate once more that the essential condition necessary to the production of the neck reflex is posturing, and in the act of posturing the extensor thrusts are important. Likewise, a position of the body is effective in producing adequate extensor thrusts, and this is in accord with the findings of Beritoff (16) with decerebrate cats, that a position is effective in producing increase extensor tone of the limbs with a change of the position of the head in space or dorsally extending the tail. The extensor thrusts are best evoked in reflex excitable specimens, and when these specimens are resting on their feet or on their backs, and not on either side. This statement is also of interest in that reflex excitability does not increase the extensor tone of the limbs unless the body of the animal is in either of these two positions. It is evident, then, that an effective position, such as resting on the feet, would be the best for the interaction of the mechanism of the reflex extensor thrusts, and that for the production of reflex-excitability, and much more so would these reflex mechanisms become effective when an increased number of body reflexes come into use as is the case when posture is attained under the latch.

Rats not manifesting reflex excitability would necessarily show contrary results from those just mentioned. Not alone is posture not produced, but, as previously stated at the end of the first trial of 30 minutes, or at the end of the sixth trial, when the problem remained unsolved, some rats were greatly exhausted. When the reflex extensor thrusts of these rats were tested at the end of the first of succeeding trials, they were found to be absent or noticeably weak. These observations are in accord with those made by Sherrington (17), in decerebrate animals, that the

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contraction phase for extensor thrusts wears out especially rapidly when the reflex excitability of the decerebrate preparation is low, and seems to wear out more rapidly than the inhibition phase. This wearing out of the contraction phase for the extensor thrust can scarcely result from changes taking place in the neural centers alone. The efficiency of the organism being notably dependent upon reflex excitability, it appears that the chemical correlates secreted at this time are subnormal in amount, and the effects of fatigue are not lessened. Gruber (18) has shown that with epinephrin profusion of the body musculature, an invigoration of the musculature is apparent. It may be supposed with a subnormal production of epinephrin, an invigoration of the body musculature does not take place. Possibly a decrease in the efficiency of the reflex musculature can more definitely be said to be traceable to a quantitative subnormal production from other endochrine glands than that of the adrenal glands. That other glands function likewise in the invigoration of striated muscles is in accord with the researches of Rogers, Cooms, and Rahe (19). Intravenous injection of non-coagulable portions of alkaline extracts of fresh thyroids, parathyroids, and adrenal glands increase the vigor of contractions of fatigued voluntary muscles. It appears that a subnormal production of a number of glands accounts for exhaustion in rats.

From these experiments on the endochrine secretions during fatigue, apparently the direct effect of the substances secreted is upon the muscle fibers and not upon neural centers. According to Pratt (20), during muscular fatigue there takes place an elimination of contractile elements. It is possible that endochrine secretions invigorate the muscle fibers that tend to be eliminated. With the appearance of fatigue and a subnormal production of endochrine secretions, an elimination of contractile elements would prevent the production of effective posture to produce the neck reflex movement.

The two reflex mechanisms, the reflex extensor thrusts of the rats limbs, and that for the production of reflex excitability, do not stand alone, for excitations in the sensory mechanism must evoke responses. The senses are directly concerned in initiating

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the development of a greater interaction of the parts of an organism. Any undeveloped condition that may exist in the external senses can not be revealed by the behavior of rats, for it is not at all times observable what external senses can be supposed really to direct progression. At times it is not certain that the movements produced are at all evoked by excitations in an external sense, but appear because the reflex mechanism for progression is undeveloped. Only when a symmetrical development of the extensor thrusts exists, is it observable that movements are directive and that the senses are at all concerned in producing them. Moreover, in such a developed condition of the thrust, the interaction of the external senses with the mechanism for the production of reflex excitability is explicit, for when rhythmic extension and flexion of the limbs is evoked by the test for them, and reflex excitability is manifest, the relation of one to the other of the reflex mechanisms is reciprocal. An existing undeveloped condition of the thrusts or of the mechanism for the production of reflex excitability, reveals a want of interaction of body parts which results in the performance of a number of movements. Sometimes it appears that the numerous movements made are the direct result of an incomplete interaction of body parts of the rats, and not due to any specific sensory excitations. The term "random" is then not inappropriately applied to movements when they can not, for a time, be effectively produced, but not for any other reason, such as the rat produces them; for when movements are at all directive on the first trial, it appears that a spread of excitations from numerous senses increase the extensor tone of integrated reflexes, and all movements, inclusive of posture, are effectively performed. When the extensor tone of the reflexes can not reach a certain point, then an excitation in the external sense of touch, of vision, or of olfaction does not produce effective responses and direct progression. The external sense, touch, does not then produce the complex integrated movement of the neck reflex. The results obtained and presented in tables 1 and 2, indicate this very clearly. Before this neck reflex movement can be produced, it is necessary that many sensory excitations be aroused by manifold sensory stimuli, or in other

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words the configuration of the problem, produce a greater interaction of body parts, so that posture under the latch can be produced. Then a peripheral stimulus from the latch, increases the tonic reflexes of the rat's body for the production of posture and the neck reflex movement.

Our idea of the production of a greater interaction of body parts is not complete without mentioning the significance with which other body parts function. Beritoff (21) has noted that in decerebrate animals, an increased rate in respiration and in circulation immediately increases the extensor tone of the limbs. The effect of an increased rate in activity in these mechanisms has been observed in rats. Actual comparisons of the rate in heart beat and in respiration in rats before and after the problem was solved revealed the fact that after the problem was solved, a decided increase in the rate of activity in these parts appeared in most rats, particularly in those rats which manifested reflex excitability. When reflex excitability was not manifest in rats and the problem remained unsolved, the rate in heart beat and in respiration was at the end of 30 minutes slightly increased or feeble. These observations were made by counting the rate of rhythm of these mechanisms before and after a trial. From these calculations, it can be said that with an increase in activity of all movements which produce a cumulative effect on the organism leading to the solution of the problem, there occurs an increase in respiratory exchange and an increase in heart rate that increases the extensor tone of the reflexes involved in posturing and in the production of the neck reflex movement. There is likewise as a result produced a greater metabolic rate and oxidations in the rat's body. Then the cumulative effect produced by numerous movements and the changes produced by them is a necessary physiological process to produce greater interaction of essential parts to produce the neck reflex movement.

In the course of training, it will be seen that a greater activity of inherent coördinated reflex movements takes place, and this is accomplished by a greater development of an interaction of many body parts. The importance of the first trial can be determined in the light of changes that take place in succeeding

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trials, particularly in the first few. There is developed, or better facilitated, at these trials, direct progression to the door of the latch-box problem from the entrance box. A great many rats were observed during the facilitation of direct progression and the production of posture, and these two movements though previously coördinated were now by training bettered for the production of the neck reflex movement. An appropriate term for the improved performance of these two movements was thought desirable, and the term directive integration was adopted. The development of directive integration may be expressed by saying direct progression and posturing have been facilitated for the production of the neck reflex.

In most cases, before directive integration is facilitated, many movements are more effectively directed to the door of the problem box. The most noticeable external sense which aids direct progression to the door is vision; for, on the second or third trial, progression is, with the aid of vision, direct to the door. In some cases when directive integration is slow in being facilitated, the latch is for some time located by touch. As soon as progression to the latch is facilitated and posturing is bettered, apparently no definite external sense aids progression to the latch. The facilitation of direct progression with the production of posture apparently comes about through the development of an interaction of body parts of the rat, rather than through the establishment of "sensory association."

In the first few trials with the facilitation of directive integration, the greatest change that occurs is not in the production of the neck reflex but in all the integrated movements that are auxiliary to it. The introduction of vision is one progressive step in the development of interaction for the facilitation of directive integration and the attainment of posture. The change is particularly noticeable in those rats in which the reflex thrusts are functionally the best. Very few movements in the second and a few succeeding trials occur before the production of posture under the latch, and the neck reflex is produced much more readily. With other rats numerous movements are still made. Posturing is excessive and relaxation from such a position with

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the production of the neck reflex is not a free and easy one. The latch is still sent out of the notch with considerable vigor. Evidently the ease with which the neck reflex is produced is associated with an early facilitation of directive integration.

It is impossible to detect any variation in the performance of the neck reflex movement, but, variations in the movements which may be regarded as coördinated with it, or which are auxiliary to it, are easily detected. When imperfect responses appear after the facilitation of directive integration, these responses can readily be observed. These responses indicate not so often an ineffective performance of the neck reflex movement, but of the movements coördinated with it. In fact all imperfect responses result from imperfect coördination of these integrated movements with that of the integrated neck reflex movement. It is advisable that the degree of facilitation of this coördination of movements in different rats be shown. To this end, an apparatus was constructed, so that accurate reaction time for the neck reflex movement, and that for directive integration could be obtained. For, if there exists any variation in the reaction time of either of these movements, there will be revealed the degree of facilitation of coördination of them. Moreover it might indicate at times a want of interaction of integrated movements and other reflex mechanistic parts of the rat.

To record the reaction time of all movements of the neck reflex and of directive integration, a specially constructed latch was attached to the problem box, and a platform with electric contacts was placed in the entrance box of the hood which covered the problem box. The platform was supported by two light springs. Two electric contacts, one at each end on the under surface of the platform were in circuit with a signal magnet and batteries. Slight pressure on the upper surface of the platform would bring the electric contact points together closing the circuit. The circuit was closed when a rat was on the platform before progression inside the hood took place, and was opened when progression from the platform occurred. Both these time intervals were recorded by a signal magnet on a kymograph. The last interval alone was used to reckon the time for the

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beginning of progression to the latch. The position of the platform in the entrance box and the position of the latch on the problem is seen in figure 1.

The latch, figure 2, consisted of a hollow aluminum sheath with an easily movable insert of hard rubber. Attached to the inside of the upper middle part of the sheath was an insulated brass lever which made electric contact with the sheath when

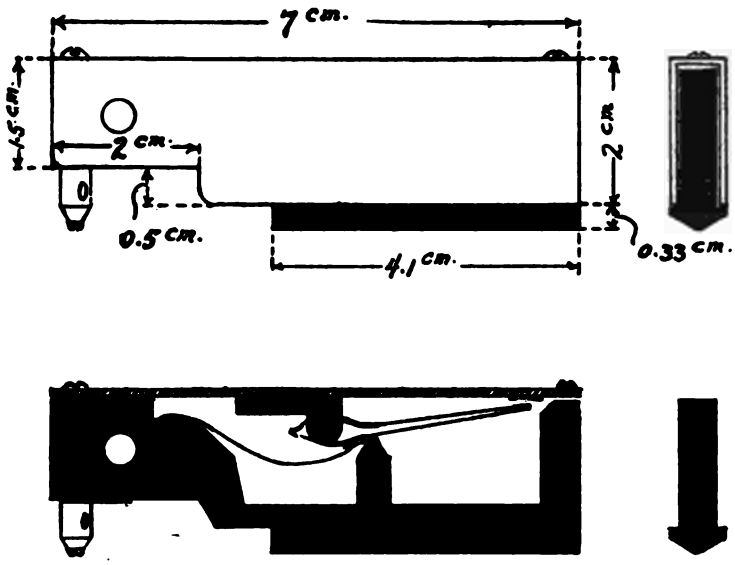

FIG. 2. THE LATCH

the hard rubber insert was pressed upward. The electric contact points were of platinum and of gold. The rubber insert was held in place by a screw. The screw also served to adjust the distance which the rubber insert would have to be moved in order to raise the lever. Attached to the insert was a curved bronze spring on which the lever rested. Slight pressure on the under surface of the insert increased the tension on the spring thereby raising the lever, making electric contact with the lever

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and the aluminum sheath. A binding post of rubber insert connected by a wire the bronze spring and the brass lever. A screw through one end of the sheath and the rubber insert attached the latch to the frame of the problem. This screw afforded means of making the second complete electric circuit, the other being made with the binding post on the rubber insert. A wire from the problem box and one from the binding post were put in circuit with batteries and another signal magnet. The reaction time for directive integration could be taken from the moment the hood was entered until the latch was touched.

Another contact point had to be attached to the problem box to record the reaction time of the neck reflex movement. A fiber button supporting a light spring made contact with the aluminum sheath of the latch when the latch was raised out of the notch. Contact points of platinum were connected in circuit with batteries and a signal magnet. The exact height of the spring was adjusted to the depth of the notch, and the height of the spring, its flexibility, and the depth of the notch were determined by experimentation. These parts were adjusted to the extent of, or to the amplitude of the neck reflex movement. Since the spring gave way freely the amplitude of this movement could not be said to have been shortened by the latch coming in contact with the spring on the rubber button. An easy movement of the neck reflex would be said to occur when the latch was raised.

The amount of pressure required to raise the latch out of the notch was also considered. A definite amount of tension on the spring which opened the door when the latch was raised out of the notch, increased the pressure that must be exerted on the latch. This slight pressure seemed favorable to the production of the neck reflex movement, for when tension on the spring was reduced, a less adequate neck reflex movement was produced, and the latch was often moved only part way out of the notch. Posture then was frequently inadequate, and this seemed to be due to lack of work against the latch. The actual pressure required to raise the latch was equivalent to 33 grams.

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Two different reaction times could with such an arrangement, be obtained: first, from the beginning of directive integration when the hood was entered until the end of it when the under surface of the latch was touched; second, from the moment the under surface was touched until the latch was moved out of the notch. Directive integration had to be made over a distance of 20 cm. For the first trial, the reaction time of these movements was taken with a Jaquet chronoscope adjusted to time intervals of $\frac{1}{5}$ of a second, and the second and succeeding trials, were taken with a tuning fork having double vibrations of $\frac{1}{50}$ of a second. Both records were obtained on the smoked drum of a kymograph. The total time required to solve the problem could be had by adding the reaction time of the movements for directive integration and for the neck reflex.

The results obtained from this part of the investigation of the integrated movements to learn the latch-box problem will be found in tables 4, 5, 6, 7, 8, and 9. In addition to the reaction time for the movements of directive integration and the neck reflex, other significant data are presented. The functional condition of the reflex thrusts from trial to trial, the degree of reflex excitability manifest, and the kind of imperfect responses made are recorded. The tests for the reflex extensor thrusts of the rat's limbs were made once prior to a trial. These tables show rather clearly the development of interaction of body parts during learning both when these parts were functionally developed and undeveloped.

The record of the first rat shown in table 4, is that of a rat with functionally well developed reflex thrusts which only at times showed a slight reduction in extensor tone. Reflex excitability was hypernormal and persisted throughout learning. Directive integration appeared in the second trial and was thoroughly facilitated on the third trial. It was maintained uninterruptedly to the very end of learning. Often sudden increases and decreases in reaction time of directive integration appeared from time to time; yet from the eighteenth trial the reaction time for it became more uniform. Irregular variations in the neck reflex movement also appeared, ranging from $\frac{3}{50}$ to $\frac{7}{50}$ of a

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TABLE 4

NUMBER	TO DOOR	NECK REFLEX	TOTAL TIME	
1	526	1	527	
2	310	6	316	To door
3	73	4	77	Directive integration
4	38	5	43	
5	59	5	64	
6	36	7	43	
7	30	6	36	
8	55	5	60	
9	43	5	48	
10	53	7	60	
11	30	4	34	
12	22	7	29	
13	26	4	30	
14	27	7	34	
15	42	7	49	
16	21	4	25	
17	19	7	26	
18	18	7	25	
19	24	5	29	
20	20	7	27	
21	26	6	32	
22	19	4	23	
23	15	5	20	
24	26	5	31	Reflex thrusts fully developed
25	29	5	34	
26	13	5	18	
27	17	5	22	
28	21	5	26	
29	28	5	33	
30	24	5	29	
31	60	7	67	
32	17	7	24	
33	30	5	35	
34	28	5	33	
35	35	6	31	
36	58	5	63	
37	15	3	18	
38	22	5	27	
39	20	4	24	
40	20	5	25	
41	22	4	26	
42	30	3	33	
43	18	3	21	

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TABLE 4—*Continued*

NUMBER	TO DOOR	NECK REFLEX	TOTAL TIME	
44	17	4	21	
45	15	4	19	
46	18	4	22	
47	14	4	18	
48	22	4	26	
49	21	6	27	Reflex thrusts fully developed
50	22	4	26	
51	19	6	25	
52	25	6	31	
53	38	7	45	
54	52	7	59	
55	21	3	24	

second, but no gradual decrease in the performance of this movement occurred. No remarks need be made of the total time required to solve the problem, for variations in them can be accounted for in the variations that appear in the reaction time for directive integration and for the neck reflex movement. The number of reaction times taken with this rat was 78, and no imperfect responses occurred throughout the trials. In order that the results obtained from this rat may appear more uniform with the remaining tables given, only the reaction times for 55 trials are presented. The records of the 23 trials which are omitted did not vary from the last 15 of the 55 trials.

The records of the rats presented in the other tables are quite different from the preceeding one. They are from rats whose reflex thrusts are functionally poorly developed, fluctuating at shorter or longer intervals. Tables 5, 6, and 7, are from rats whose reflex thrusts appear from the very first trial poorly developed; tables 8 and 9 are records of rats whose reflex thrusts fluctuated often and greatly. The records of these two groups of rats present different results. The rats of the first group possessed in the first or second trial an ipsilateral fore reflex thrust, when the head was passively bent to one side, usually none when the head was dorsally extended 45 degrees of the midline of the body, and weak or absent hind reflex thrusts when the tail was

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dorsally extended. On the other hand, the records from the second group show that the reflex thrusts fluctuated markedly and often. At times nearly all the reflex thrusts could be evoked. In addition, the first rat of this group showed evident fluctuations in reflex excitability. Fluctuations in excitability have frequently been observed, and then accompanied at times with marked fluctuations in reflex extensor thrusts. To such an extent did fluctuations occur in rats whose record is presented in table 8, that the reaction time for directive integration and the neck reflex movements could not at times be obtained. Wandering progression from the entrance box occurred instead of direct progression to the door, and the reaction times were impossible to obtain by one revolution of the kymograph. Consequently, blank spaces occur in the table when reflex excitability fluctuated. Frequently great increases in the reaction times for directive integration and for the neck reflex appear. With the second rat of this group instead of progression being direct to the latch, it was often around the problem before the latch was raised. Again the reaction time for directive integration and for the neck reflex could not be obtained. Though reflex excitability was manifest in this rat, there was great difficulty in posturing under the latch to produce the neck reflex movement, and this appears to be due to active and frequent fluctuations of the reflex extensor thrusts.

The results from the rats presented in tables 8 and 9 are different from those obtained from any rats experimented with, and this shows that as far as these experiments go, they do not exhaust what may still occur when learning the latch-box problem, particularly is this so when reflex excitability can not readily be judged to be hyponormal. The individual functional condition of rats varies greatly. The record of the first of these two rats shows that though reflex excitability was hypernormal in the first trial, activity was constant but slow. The problem was solved on the first day, but in later trials reflex excitability became after a marked fluctuation hyponormal. The record of the second rat shows that reflex excitability was always manifest, the problem was solved on the first day, but learning was

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impossible. Posture was difficult to facilitate when rapid movements direct to the latch from the entrance box occurred. Previously in table 2 it was shown that when the problem was solved on the fifth or sixth day, learning appeared impossible.

Any great variation in the results in these tables is the result of the functional condition of the rat's reflex parts, namely, that of the reflex thrusts, that for the production of reflex excitability, and possibly others. Their functional condition determines the results obtained. The most important things to be noted are the reaction time of the neck reflex movement, that for directive integration, and when these movements are facilitated. It is also of interest to note the conditions that arise during the facilitation of them. All these things deserve separate consideration for they are significant in learning.

The reaction time of the neck reflex movement varies from trial to trial. Decided increases as well as decreases occur, and these are conditioned by changes in the functional condition of the rats. These tables show that when the functional condition is the best, the reaction time of the neck reflex movement varies the least, and the variation is proportionally greater when the reflex mechanisms fluctuate. The reaction time for the neck reflex varies the least in table 4 where the functional condition of the rat is the best. It varies from $\frac{3}{5}\frac{3}{0}$ to $\frac{7}{5}\frac{7}{0}$ of a second. Where in other tables the reflex thrusts are stated to be poorly developed, variations in the reaction time are greater, being anywhere from $\frac{3}{5}\frac{3}{0}$ to $\frac{2}{5}\frac{4}{0}$ of a second. The reaction time varies in table 5 from $\frac{3}{5}\frac{3}{0}$ to $\frac{1}{5}\frac{6}{0}$, table 6 from $\frac{3}{5}\frac{3}{0}$ to $\frac{1}{5}\frac{4}{0}$, table 7 from $\frac{3}{5}\frac{3}{0}$ to $\frac{1}{5}\frac{8}{0}$, table 8 from $\frac{3}{5}\frac{3}{0}$ to $\frac{2}{5}\frac{4}{0}$ of a second. At the end of a series of trials, the reaction times become more uniform, and for the last 10 trials, they average about $\frac{3}{5}\frac{5}{0}$ of a second. There is no gradual decrease in the reaction time for the neck reflex movement, but the reaction time becomes much more regular as the trials increase in number.

A greater difference in the reaction time for directive integration occurs, and here the reaction time gradually decreases from trial to trial. This seems to indicate that a greater difficulty occurs in the facilitation of directive integration than to produce

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TABLE 5

NUMBER	TO DOOR	NECK REFLEX	TOTAL	REFLEX THRUSTS			REFLEX EXCITABILITY
				Fore	45 degrees	Hind	
1	1640	4	1644	Both	None	B. str.	Hypernormal
2	2520	8	2528	Both	None	B. str.	Hypernormal
3	847	7	854	Both	None	B. str.	Hypernormal
4	1874	7	1881	Left	None	B. wk.	Hypernormal
5	144	6	150	Left	None	B. wk.	Hypernormal
6	244	5	249	Left	None	B. wk.	Hypernormal
7	194	5	199	Left	None	B. wk.	Hypernormal
8	66	9	75*	Left	None	B. wk.	Hypernormal
9	26	6	32	Left	None	B. wk.	Hypernormal
10	38	7	45	Left	None	B. wk.	Hypernormal
11	34	5	39	Left	None	B. wk.	Hypernormal
12	29	5	34	Left	None	None	Hypernormal
13	18	5	23	Left	None	None	Hypernormal
14	20	6	26	Left	None	None	Hypernormal
15	82	7	89†	Left	None	None	Hypernormal
17	62	5	67	Left	None	None	Hypernormal
18	23	5	28	Left	Right	None	Hypernormal
19	360	16	376‡	Left	None	None	Hypernormal
20	22	5	27	Left	None	None	Hypernormal
21	24	4	28	Left	None	None	Hypernormal
22	15	8	23	Left	None	None	Hypernormal
23	31	5	36	Left	None	None	Hypernormal
24	37	5	42†	Left	None	None	Hypernormal
25	31	3	34	Left	None	None	Hypernormal
26	73	8	41	Left	None	None	Hypernormal
27	18	5	23	Left	None	None	Hypernormal
28	21	6	27	Left	None	None	Hypernormal
29	24	6	27	Left	None	None	Hypernormal
30	18	5	23	Left	None	None	Hypernormal
31	21	6	27	Left	None	None	Hypernormal
32	14	4	18	Left	None	None	Hypernormal
33	17	3	20	Left	None	None	Hypernormal
34	18	6	24	Left	None	None	Hypernormal
35	16	6	22	Left	None	None	Hypernormal
36	14	4	18	Left	None	None	Hypernormal
37	13	4	17	Left	None	None	Hypernormal
38	25	4	29	Left	None	None	Hypernormal
39	23	4	27	Left	None	None	Hypernormal
40	26	9	35	Left	None	None	Hypernormal
41	20	4	24	Left	None	None	Hypernormal
42	140	4	144§	L. str.	None	None	Hypernormal
43	12	4	16	L. str.	None	None	Hypernormal

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TABLE 5—*Continued*

NUMBER	TO DOOR	NECK REFLEX	TOTAL	REFLEX THRUSTS			REFLEX EXCITABILITY
				Fore	45 degrees	Hind	
44	39	7	46	L. str.	None	None	Hypernormal
45	12	4	16	L. str.	None	None	Hypernormal
46	21	4	25	L. str.	None	None	Hypernormal
47	18	4	22	L. str.	None	None	Hypernormal
48	13	4	17	L. str.	None	None	Hypernormal
49	22	4	26	L. str.	None	None	Hypernormal
50	11	4	15	Left	None	None	Hypernormal
51	10	4	14	Left	None	None	Hypernormal
52	14	5	19	Left	None	None	Hypernormal
53	20	5	25	Left	None	None	Hypernormal

* Directive integration.
† Two pushes.
‡ To one side of latch.
§ Missed latch.

the neck reflex movement. In table 4, where the reflex thrusts are stated as functionally developed, direct progression to the latch is indicated as appearing on the second trial, and directive integration is facilitated on the third trial; and in other tables, where the reflex mechanisms are indicated as functionally undeveloped, it is facilitated at any trial from the fourth to the fourteenth. In tables 8 and 9, where marked fluctuations in the extensor thrusts and in manifestations of reflex excitability are stated, directive integration may be seen to disappear for one or more trials then reappear. When directive integration is disrupted, progression from the entrance box may be part way to the door, then rapidly to one side of the problem, around it, or it may be slow and wandering in any direction. In all cases, with the exception of the conditions revealed in table 9 where it is seen that directive integration can not be facilitated, towards the end of learning, the reaction time for directive integration becomes more uniform, and with it that of the neck reflex movement. The shortest reaction time for directive integration is seen in table 6 to be $\frac{1}{10}$ of a second. When the reaction time occurs repeatedly from $\frac{19}{100}$ to $\frac{25}{100}$ of a second, then its production has become fairly constant.

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TABLE 6

NUMBER	TO DOOR	NECK REFLEX	TOTAL	REFLEX THRUSTS			REFLEX EXCITABILITY
				Fore	45 degrees	Hind	
1	1294	2	1296	Both	Both	Strong	Hypernormal
2	3560	8	3568	Right	None	Strong	Hypernormal
3	975	14	989*	Right	None	Strong	Hypernormal
4	219	5	224†	Right	None	Strong	Hypernormal
5	221	4	225‡	Right	None	B. wk.	Hypernormal
6	88	5	93	Right	None	B. wk.	Hypernormal
7	87	5	92	Right	None	B. wk.	Hypernormal
8	91	5	96†	Right	None	B. wk.	Hypernormal
9	51	5	56	Right	None	B. wk.	Hypernormal
10	17	5	22	Right	None	B. wk.	Hypernormal
11	39	9	48	Right	None	B. wk.	Hypernormal
12	587	5	592*	Right	None	B. wk.	Hypernormal
13	29	5	34	Right	None	B. wk.	Hypernormal
14	58	4	62	Right	None	B. wk.	Hypernormal
15	43	3	46	Right	None	B. wk.	Hypernormal
16	39	5	44	Right	None	B. wk.	Hypernormal
17	29	5	34	Right	None	B. wk.	Hypernormal
18	29	5	34	Right	None	None	Hypernormal
19	13	5	18	Right	None	None	Hypernormal
20	38	6	44§	R. wk.	None	None	Hypernormal
21	37	5	42	R. wk.	None	None	Hypernormal
22	20	4	24	R. wk.	None	None	Hypernormal
23	17	5	22	R. wk.	None	None	Hypernormal
24	84	3	87	R. wk.	None	None	Hypernormal
25	159	5	164¶	R. wk.	None	None	Hypernormal
26	55	12	67§	R. very wk.	None	None	Hypernormal
27	22	5	27	R. very wk.	None	None	Hypernormal
28	27	5	32	R. very wk.	None	None	Hypernormal
29	70	8	78	R. very wk.	None	None	Hypernormal
30	16	5	21	R. very wk.	None	None	Hypernormal
31	20	5	25	R. very wk.	None	None	Hypernormal
32	16	4	20	R. very wk.	None	None	Hypernormal
33	17	5	22	R. very wk.	None	None	Hypernormal
34	16	5	21	R. verk wk.	None	None	Hypernormal
35	80	5	85†	R. very wk.	None	None	Hypernormal
36	16	4	20	R. wk.	None	None	Hypernormal
37	14	4	18	None	None	None	Hypernormal
38	17	4	21	R. very wk.	None	None	Hyeprnormal
39	36	8	44	R. very wk.	None	None	Hypernormal
40	19	4	23	R. very wk.	None	None	Hypernormal
41	26	4	30	R. very wk.	None	None	Hypernormal
42	11	4	15	R. verk wk.	None	None	Hypernormal

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TABLE 6—*Continued*

NUMBER	TO DOOR	NECK REFLEX	TOTAL	REFLEX THRUSTS			REFLEX EXCITABILITY
				Fore	45 degrees	Hind	
43	18	5	23	R. very wk.	None	Weak	Hypernormal
44	24	5	29	R. very wk.	None	Weak	Hypernormal
45	30	5	35	R. very wk.	None	Weak	Hypernormal
46	25	7	32	R. very wk.	None	Weak	Hypernormal
47	20	5	25	R. very wk.	None	Weak	Hypernormal
48	23	4	27	R. very wk.	None	Weak	Hypernormal
49	22	4	26	R. very wk.	None	Weak	Hypernormal
50	19	4	23	R. very wk.	None	Weak	Hypernormal
51	20	4	24	R. very wk.	None	Weak	Hypernormal
52	19	4	23	R. very wk.	None	Weak	Hypernormal
53	37	3	30	R. very wk.	None	Weak	Hypernormal

* Three pushes.
† Two pushes.
‡ Directive integration.
§ Missed latch.
¶ To side of latch.

The variations in the reaction time of the neck reflex movement and in directive integration is due to the difficulty at times to produce posture, and in the production of which the extension of the rat's limbs is important. For effective posturing of the body under the latch, the extensor thrusts must to a degree be developed, and fluctuations of them must not occur. The significance of posturing can be understood from what takes place in relaxation from posture to produce the neck reflex movement. After posturing and the neck reflex is produced, the fore quarters are raised and the hind quarters slightly lowered. The forelimbs are extended and the hind flexed. In some cases, before progression to enter the problem box occurs, the body of the rat is momentarily maintained in a posture with the fore quarters raised and the hind lowered. As previously stated, great variation is shown in the extent of posturing and of relaxation of the body to produce the neck reflex movement, and this variation results from the different degrees of the development of the unequal antagonistic action of extension and flexion of the rat's limbs.

This last statement would indicate that all perfect responses are made before the latch when posturing is difficult to produce,

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TABLE 7

NUMBER	TO DOOR	NECK REFLEX	TOTAL	REFLEX THRUSTS			REFLEX EXCITABILITY
				Fore	45 degrees	Hind	
1	936	5	941*	Left	None	Strong	Hypernormal
2	2520	8	2528	Left	None	Strong	Hypernormal
3	1119	6	1125	L. wk.	None	Weak	Hypernormal
4	78	5	83†	L. wk.	None	Strong	Hypernormal
5	82	7	89*	None	None	Strong	Hypernormal
6	58	9	67*	None	None	Strong	Hypernormal
7	53	5	58	None	None	Strong	Hypernormal
8	36	8	44*	None	None	Strong	Hypernormal
9	33	5	38*	None	None	Strong	Hypernormal
10	28	8	36*	None	None	Strong	Hypernormal
11	150	5	155*	None	None	Strong	Hypernormal
12	37	8	45‡	None	None	Strong	Hypernormal
13	80	5	85	None	None	Strong	Hypernormal
14	21	5	26‡	None	None	Strong	Hypernormal
15	29	6	35	None	None	Strong	Hypernormal
16	18	5	23	None	None	Strong	Hypernormal
17	34	4	38	None	None	Strong	Hypernormal
18	25	3	28	None	None	Strong	Hypernormal
19	25	12	37	None	None	Strong	Hypernormal
20	41	5	46	None	None	Strong	Hypernormal
21	61	4	65	None	None	Strong	Hypernormal
22	18	4	22	None	None	Strong	Hypernormal
23	44	5	49	None	None	Strong	Hypernormal
24	32	5	37	None	None	Strong	Hypernormal
25	72	5	77	None	None	Strong	Hypernormal
26	40	5	45‡	None	None	Strong	Hypernormal
27	29	5	34	None	None	Strong	Hypernormal
28	195	9	204	R. wk.	None	Strong	Hypernormal
29	41	12	53*	None	None	Strong	Hypernormal
30	36	5	41	None	None	Strong	Hypernormal
31	27	4	31	None	None	Strong	Hypernormal
32	29	4	33	None	None	Strong	Hypernormal
33	25	4	29	L. wk.	None	Strong	Hupernormal
34	18	5	23	L. wk.	None	Strong	Hypernormal
35	16	6	22	L. wk.	None	Strong	Hypernormal
36	31	5	36	L. wk.	None	Strong	Hypernormal
37	30	5	35	L. wk.	None	Strong	Hypernormal
38	44	13	57	L. wk.	None	Strong	Hypernormal
39	32	5	37	L. wk.	None	Strong	Hypernormal
40	27	5	32	L. wk.	None	Strong	Hypernormal
41	15	5	20	None	None	Strong	Hypernormal
42	20	4	24*	None	None	Strong	Hypernormal
43	22	4	26	None	None	Strong	Hypernormal

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TABLE 7—*Continued*

NUMBER	TO DOOR	NECK REFLEX	TOTAL	REFLEX THRUSTS			REFLEX EXCITABILITY
				Fore	45 degrees	Hind	
44	18	4	22	None	None	Strong	Hypernormal
45	18	4	22	None	None	Strong	Hypernormal
46	31	4	35	None	None	Strong	Hypernormal
47	18	5	23	None	None	Strong	Hypernormal
48	18	3	21	None	None	Strong	Hypernormal
49	20	4	24‡	None	None	Strong	Hypernormal
50	16	4	20	None	None	Strong	Hypernormal
51	63	5	68	None	None	Strong	Hypernormal
52	18	4	22	None	None	Strong	Hypernormal
53	18	4	22	None	None	Strong	Hypernormal

* Two pushes.
† Directive integration.
‡ Missed latch.

and that fluctuations in the reflex thrusts are a disturbing factor in posturing. Fluctuations, then, are the production of imperfect responses. Usually these responses occur on the day on which these fluctuations are noted to take place, but they may appear the day after. At times these responses are not conditioned by a fluctuation in a single ipsilateral thrust, but by a fluctuation in the coördinate action of the fore and the hind limbs. Sometimes imperfect responses occur when no extensor thrusts can be evoked in any of the limbs, and in such cases fluctuations in the neural centers of the cord for the production of these thrusts undoubtedly take place, for the reappearance of even a weak thrust will often produce an imperfect response. It is also possible that a fluctuation in the thrusts, or a greater or a more effective coördination of the limbs is favorable to posturing, and imperfect responses will not appear. It is difficult to judge what condition of the reflex thrusts facilitiates posturing under the latch.

The facilitation of posture requires, as it must, a development of an interaction of many body parts. Interaction is evidenced in the widespread changes that must take place when reflex excitability is manifest during learning and when this manifestation fluctuates and wandering movements appear; it is also

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TABLE 8

NUMBER	TO DOOR	NECK REFLEX	TOTAL	REFLEX THRUSTS			REFLEX EXCITABILITY
				Fore	45 degrees	Hind	
1	1280	2	1282	Both	Right	Strong	Hypernormal
2		Unsolved		Right	None	Strong	Hyponormal
3	360	7	367	Both	None	Strong	Hyponormal
4	296	9	304	Both	None	Strong	Hyponormal
5	56	10	*66**	L. str.	None	Strong	Hypernormal
6	90	7	97	L. wk.	None	Strong	Hypernormal
7	60	7	67	None	None	Strong	Hypernormal
8	37	5	42	L. wk.	None	Strong	Hypernormal
9	48	5	53	L. wk.	None	Strong	Hypernormal
10	40	4	44	B. wk.	None	Strong	Hypernormal
11	25	5	30	B. wk.	None	Strong	Hypernormal
12	69	4	73†	Right	None	Strong	Hypernormal
13	38	5	43	None	None	Strong	Hypernormal
14	37	4	41	None	None	Strong	Hypernormal
15	22	6	28	None	None	Strong	Hypernormal
16	26	6	32	None	None	Strong	Hypernormal
17	28	5	33	None	None	Strong	Hypernormal
18	37	5	42	None	None	Strong	Hypernormal
19	18	5	23	R. wk.	None	Strong	Hypernormal
20	37	5	42	R. wk.	None	Strong	Hypernormal
21	289	5	294	R. wk.	None	Strong	Hyponormal
22	880	18	898†	None	None	Strong	Hyponormal
23				None	None	Strong	Hyponormal
24	19	5	24	None	None	Strong	Hypernormal
25	201	24	225	Right	None	None	Hypernormal
26	40	5	45	Right	None	None	Hypernormal
27	39	11	50	Right	None	None	Hypernormal
28	30	10	40	Right	None	None	Hypernormal
29	20	5	25	Right	None	Strong	Hypernormal
30	22	5	27	Right	None	Strong	Hypernormal
31	21	7	28	None	None	Strong	Hypernormal
32	25	5	30†	R. wk.	None	Strong	Hypernormal
33	20	3	23	R. wk.	None	Strong	Hypernormal
34	15	4	19	R. wk.	None	Strong	Hypernormal
35	33	5	38	R. wk.	None	Strong	Hypernormal
36	30	5	35	R. wk.	None	Strong	Hypernormal
37				R. wk.	None	Strong	Hyponormal
38				R. wk.	None	Strong	Hyponormal
39	28	8	36	R. wk.	None	Strong	Hypernormal
40	27	12	39	R. wk.	None	Strong	Hypernormal
41	191	18	209†	R. wk.	None	Strong	Hypernormal
42	21	5	26‡	R. wk.	None	Strong	Hypernormal

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TABLE 8—*Continued*

NUMBER	TO DOOR	NECK REFLEX	TOTAL	REFLEX THRUSTS			REFLEX EXCITABILITY
				Fore	45 degrees	Hind	
43	31	6	37	R. wk.	None	Strong	Hypernormal
44	14	7	21‡	R. wk.	None	Strong	Hypernormal
45	22	6	28	R. wk.	None	Strong	Hypernormal
46	18	5	23	R. wk.	None	Strong	Hypernormal
47				R. wk.	None	Strong	Hyponormal
48	233	9	242	R. wk.	None	Strong	Hyponormal
49	14	5	19	R. wk.	None	Strong	Hyponormal
50	30	5	35	R. wk.	None	Strong	Hyponormal
51	16	3	19	R. wk.	None	Strong	Hyponormal
52	14	5	19	R. wk.	None	Strong	Hyponormal

* Directive integration.
† Missed latch.
‡ Two pushes.

recognized in posturing when evidently many integrated, coördinate, movements are involved, and in the disruption of these movements when fluctuations in the extensor thrusts occur. Interaction is again indicated, when in excessive posture a greater coördinate action of the body musculature is demanded, and in the necessity for reflex excitability to be manifest for greater functioning of the entire body. When a greater functioning of the entire rat's body is required, it is not a bit surprising that fluctuations in the extensor thrusts produce imperfect responses disrupting developed interaction of body parts.

In the development of interaction there is evidence of a change taking place in the rat, and this is seen in the decrease in the extensor tone of all limbs when these limbs are functionally undeveloped. When this decrease is impossible, then learning is difficult or impossible. This decrease in the reflex tone occurs in most rats, and particularly in the fore limbs, but it may occur in the hind limbs. Of the two ipsilateral fore reflex extensor thrusts, the one showing a weaker extensor tone disappears and its contralateral remains, but in a much weaker condition. This decrease in extensor tone is shown in tables 5 and 7, where of the two fore thrusts that were evoked in the first trial, an ipsilateral fore reflex thrust alone remains. Invari-

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TABLE 9

NUMBER		REFLEX THRUSTS			REFLEX EXCITABILITY
		Fore	45 degrees	Hind	
1	Solved	Both	None	Strong	Hypernormal
2	Solved	Right	None	Strong	Hypernormal
3	Solved	Both	None	Strong	Hypernormal
4	Solved	Both	None	Strong	Hypernormal
5	Solved	Both	R. wk.	None	Hypernormal
6	Solved	Right	R. wk.	None	Hypernormal
7	Solved	Right	R. wk.	None	Hypernormal
8	Solved	Right	None	None	Hypernormal
9	To latch to side of the problem	Both	None	None	Hypernormal
10	To latch to side of the problem	B. wk.	None	None	Hypernormal
11	To latch to side of the problem	R. wk.	None	None	Hypernormal
12	To latch to side of the problem	R. wk.	None	None	Hypernormal
13	Directive integration	R. wk.	None	None	Hypernormal
14	" "	B. wk.	None	None	Hypernormal
15	" "	B. wk.	None	None	Hypernormal
16	" "	B. wk.	None	None	Hypernormal
17	" "	B. wk.	None	None	Hypernormal
18	" "	B. wk.	None	None	Hypernormal
19	" "	B. wk.	None	None	Hypernormal
20	" "	B. wk.	None	None	Hypernormal
21	" "	B. wk.	None	None	Hypernormal
22	" "	Right	None	None	Hypernormal
23	" "	Right	None	None	Hypernormal
24	" "	Right	None	None	Hypernormal
25	" "	Right	None	None	Hypernormal
26	To the side of problem	Both	None	None	Hypernormal
27	Around the problem	Right	None	None	Hypernormal
28	Around the problem	Right	None	None	Hypernormal
29	Around the problem	Right	None	None	Hypernormal
30	Around the problem	Right	R. wk.	Weak	Hypernormal
31	Directive integration	Right	R. wk.	Weak	Hypernormal
32	3 Pushes	Right	None	Weak	Hypernormal
33	Directive integration	Both	None	Weak	Hypernormal
34	Missed the latch	None	None	Weak	Hypernormal
35	Directive integration	Right	None	Weak	Hypernormal
36	" "	Right	None	Weak	Hypernormal
37	" "	Right	None	Weak	Hypernormal
38	" "	Right	None	None	Hypernormal
39	Two pushes	Right	None	None	Hypernormal
40	Around problem; two pushes	R. wk.	None	None	Hypernormal
41	Around problem; two pushes	R. wk.	None	None	Hypernormal
42	Around problem; two pushes	R. wk.	None	None	Hypernormal
43	Missed the latch	R. wk.	None	None	Hypernormal

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TABLE 9—*Continued*

NUMBER		REFLEX THRUSTS			REFLEX EXCITABILITY
		Fore	45 degrees	Hind	
44	Around the problem	R. v. wk.	None	None	Hypernormal
45	Around the prolbem	R. v. wk.	None	None	Hypernormal
46	Around the problem	R. v. wk.	None	None	Hypernormal
47	Around the problem	R. v. wk.	None	None	Hypernormal
48	Around the problem	None	None	None	Hypernormal
49	Around the problem	Right	None	None	Hypernormal
50	Around the problem	Right	None	None	Hypernormal
51	Around the problem	Right	None	None	Hypernormal
52	Around the problem	None	None	None	Hypernormal
53	Directive integration; missed latch	Right	Right	Right	Hypernormal
54	Around the problem	Right	Right	Right	Hypernormal

ably when in the beginning of learning only one ipsilateral fore reflex thrust can be evoked, it soon disappears, possibly to reappear at intervals as is seen in table 6. In some instances instead of an actual decrease in the extensor tone or the total disappearance of the extensor thrust, flexion of the limb is evoked. Thus a distinction must be made between extensor tone and the limb remaining limp and immovable, and extension of the limb passing into flexion of it. When the rat's limbs remain immovable, neither set of antagonistic muscles contracts. The previous marked asymmetry of the body of the rat which existed in the beginning of learning, disappears as a result of a reduction in the extensor tone of the limbs and a partial symmetry is established. This change with the use of many muscles to attain posture is adequate for the completion of learning.

The almost daily decreases and increases in reaction time for the neck reflex movement and for directive integration can now be accounted for by the occurrence of active fluctuations disrupting developed interaction of body parts. When noticeable increases in reaction time have occurred in directive integration, then disruptions of this movement have carried the animal some distance from the door of the problem box, and when slight increases in reaction time have occurred, imperfect responses have appeared before the latch. When these responses

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appeared before the latch, an ineffective neck reflex movement was performed. At times increases are due to two or more ineffective neck reflexes produced to raise the latch. When no imperfect responses were noted, as was the case with two rats, the increases and the decreases must result from no decided fluctuations in the rat's reflex mechanisms interacting with other body parts.

With the occurrence of imperfect responses, it is difficult to say when learning is complete, for it is difficult to obtain many consecutive perfect records. Usually from 5 to 20 are produced. The absence of imperfect responses is not always an indication that learning or the development of interaction of body parts is complete; for the first 15 perfect responses recorded in table 4 can not be regarded as a safe criterion that learning is complete. Decreases in reaction time did not take place until the sixteenth trial, and, if training had stopped at this time, there would be no certainty as to what would have been the reaction time after this trial, and whether interaction had been developed. The best criterion indicating that learning is at an end, is, when after a reduction in the reaction time for each trial, no further reduction is possible. Only slight changes in time for each trial appear.

In this investigation of learning in the latch-box problem, little support is to be had for prevalent theories of learning based upon the working concept of "trial and error" or upon the performance of forced movements or tropisms. The production of forced movements is dependent upon the existence of a bilateral symmetry existing in animals, and the majority of rats are asymmetrically developed, showing an unequal antagonistic action of the limbs. As a result forced movements can not be considered in any theory of learning. In respect to other theories of learning, very little support for them can be said to exist. It has previously been stated that upon the observation of the first trial in learning the actual support for the "trial and error" concept of learning with its twofold division of movements into "successful" and "unsuccessful" rests. In the investigation of the first trial in solving the latch-box problem, it is found that

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an antithetical division of movements is a needless one. In a functionally developed rat, every movement made, results from an effective adjustment to the outside of the problem box, inside of the hood which covers the box, or from an effective adjustment leading to the raising of the latch. The neck reflex movement which raises the latch does not, accordingly, appear because of the production of a number of movements, for every movement is effectively performed at some configurated part of the problem box. When the functional condition of the rat is not the best, then ineffective and effective adjustments to configurated parts of the problem box are in evidence. These different adjustments can not be regarded as antithetical sorts, for both are not produced in all rats. Effective adjustment to the problem box, or solving it, may or may not take place. The conditions which determine ineffective adjustment can not be adequately expressed by saying that the "successful" movement did not appear, and that it appears only in a few cases.

Present theories of learning can not account for the divergent results obtained in the first trial. The most difficult thing for the adherents to a definite theory of learning to explain, is the number of times the problem box remains unsolved. In such instances repeated touching of the underside of the latch evidently has no definite effect on the nervous system, neither increasing the so-called permeability of the synapses, nor selecting neural arcs. This fact seems to show conclusively that definite sensory excitations are not so essential to the solving of the problem in the first trial, as the working concept of "trial and error" holds to be necessary to produce the "successful" movement.

In succeeding trials, when the problem is solved, an association of the senses does not account for the results obtained. It would be difficult to account for the fluctuations in the unequal antagonistic action of the rat's limbs conditioning imperfect responses by the assumption that definite sensory excitations produce these fluctuations. The imperfect responses seem not to be conditioned by definite sensory excitations, but by changes in the nervous system which produce fluctuations in the rat's

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limbs. In addition, directive integration can not be said to be facilitated by an association of the senses, when its facilitation is shown to be primarily dependent upon the functional condition of the entire organism. "Sensory association," if established in learning, should, in a conclusive way, modify the nervous system and remove the fluctuations of the extensor thrusts which produce imperfect responses, disrupting directive integration from time to time. The extensor thrusts play an exceedingly important part in learning. All the movements made by a rat seem to be associated with the fundamental movement of extension and flexion of the limbs in progression, and if the nervous system is modified in a definite way in learning, this movement ought first to be developed in each limb and the unequal antagonistic action of the limbs disappear, before any changes take place in the nervous system. Fluctuations and the undeveloped condition of the extensor thrusts persist. Sensory excitations do not, it seems, predominantly function in learning so as to modify in any definite way the nervous system; that is, there is no specific "sensory control" of movements as is implied in all present theories of learning.

With no direct evidence of a definite modification in the nervous system taking place in learning, it is difficult to assume that the reappearance of specific sensory excitations produce in succeeding trials a fixation or a retention of a "successful" movement. With an effective adjustment to every part of the environment outside of the problem box and an exaggerated performance of progression, and the neck reflex movement for a greater adjustment to the problem box to raise the latch, it is hardly necessary to posit any additional view that new movements are fixated. The exaggerated movements persist, because they are, of all the movements made, the most effective in an adjustment to a particular configurated part of the problem box, when previously other parts of the box have been effectively adjusted to. The occasional use of the teeth to raise the latch is another method but it is much less effective. Another consideration, is that progression and the neck reflex movement are inherently conjoined, and it is not necessary to assume that a

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fixation of a new movement takes place. The movements required to solve the latch-box problem are exaggerated movements, identical with progression, and the moving of light objects about with the muzzle. Physiologically, these movements are mechanically efficient, and their efficiency is increased by training, as any physiological process is by activity when interacting with other body parts.

(To be continued.)

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BOOK REVIEWS

WATSON, JOHN B. *Psychology from the Standpoint of a Behaviorist.* Philadelphia, 1919, xiv + 429 pages.

The aim of this book seems to be to present a common-sense psychology freed from the hampering restrictions of uncontrolled subjectivism and parallelistic or other incomprehensible metaphysics. The novitiate, we are told, has to endure no holy vigil nor pass through secret initiation ceremonies before seriously beginning his work.

Just what may be the special province of psychology as here delimited, however, remains obscure to the uninitiated reader despite numerous passages intended to clarify the question. The "matter of environmental adjustment" (p. 9) has been claimed since before the day of Herbert Spencer as the most fundamental of all general biological activities. Again, it will be hard to find a clearer statement of the aims of neuro-physiology than this (p. 10): "The goal of psychological study is the ascertaining of such data and laws that, given the stimulus, psychology can predict what the response will be; or, on the other hand, given the response, it can specify the nature of the effective stimulus."

The formal differentiation of this psychology from physiology (p. 19) leaves the physiologist somewhat in the condition of the man who has not only been robbed of the key to his treasure chest but sharply rapped on the head to boot. "Physiology teaches us concerning the functions of the special organs, ... but nowhere in physiology do we get the organism, as it were, put back together again and tested in relation to its environment as a whole." It is true that "all the king's horses and all the king's men" in the service of physiology have not yet succeeded in accomplishing this feat, yet the very creditable advances toward this consummation in the volume before us read very like excellent experimental physiology.

Following the first chapter on Problems and Scope of Psychology is a chapter on Psychological Methods in which some of the usual physiological methods as employed in psychological laboratories are briefly described, together with sections on Verbal Report Methods and Methods of Testing. In the next chapter (The Receptors and their Stimuli) 65 pages are devoted to the elements of sense physiology.

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Chapters iv and v, comprising 81 pages, include The Elementary Facts about the Neuro-physiological Basis of Action and The Organs of Response: Muscles and Glands. These two chapters (like those which precede them) are, of course, merely the scaffolding for the psychological structure and in the Preface it is stated that they can be omitted without injuring the continuity of the text. "It is realized that only the specially interested [and qualified, it may be added] student will master them."

The reviewer has had considerable experience (not all of it gratifying to his complacency) in teaching the anatomy and physiology of the nervous system to students of psychology with meager biological preparation, and he is convinced that few pedagogical problems offer more of difficulty—and that few present so large an assortment of miserable failures. The attempt to teach details of brain anatomy, including conduction pathways, etc., without actual and prolonged laboratory contact with the material is futile. This Doctor Watson recognizes (p. 113).

It remains true, however, that there are very numerous neurological subjects of great importance from the behaviorist's standpoint which can be presented in untechnical form without the encumbrance of the benumbing jargon affected by the neurologists. The student of elementary psychology would probably be very little interested in the fact that the central canal of the spinal cord and the fourth ventricle communicate at the foramen of Magendie (p. 126), even if it were true; but he has a legitimate curiosity about reflex patterns and their mechanisms, the significance of final common paths, the apparatus of summation and reinforcement, the integrating values of sympathetic, spinal, bulbar, thalamic, and cortical centers, and a host of other topics which can be simply explained in plain English and which have very obvious "behavioristic" significance. This kind of elementary neurology has not yet been written and the author who succeeds in doing it will perform a real service.

The chapter on the nervous system is not well organized from the functional point of view and is unfortunately marred by a number of errors which would be serious if the readers were expected to take the chapter seriously. The discussion of the organs of response is better written, though it will certainly prove hard reading for students with little preparation in anatomy and physiology.

"Human action as a whole," we read (p. 194), "can be divided into *hereditary* modes of response (emotional and instinctive), and *acquired*

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modes of response (habit)." Emotion and instinct each receives one chapter and the remaining four chapters are devoted to various phases of habit.

"An emotion is an hereditary 'pattern-reaction' involving profound changes of the bodily mechanism as a whole, but particularly of the visceral and glandular systems" (p. 195). Instinct is defined "as an hereditary pattern reaction, the separate elements of which are movements principally of the striped muscles" (p. 231).

The chapter on instinct is an important original contribution, containing a wealth of new observations and laboratory studies on newborn and other very young children. Few aspects of the study of man have been more neglected than this, and none will yield more immediate rewards of patient and skilful inquiry, as Doctor Watson's valuable results illustrate.

Habit, as indicated above, includes the rest of psychology. "Any definite mode of acting, either explicit or implicit in character, not belonging to man's hereditary equipment, must be looked upon as a habit" (p. 270). Chapter viii includes an analysis of the explicit bodily habits with new observations on habit formation in children and a summary of this process in adults.

Implicit habit systems can be observed only with the aid of instruments. "When we study implicit bodily processes we are studying *thought*" (p. 326). The language habits dominate this field and to these most of Chapter ix is devoted. In the long discussion of the anatomical basis of language much is said about the larynx, muscles and other motor apparatus, but not a word about brain and nerve.

In thus leaving the impression here, as elsewhere, that "implicit" processes are necessarily peripherally expressed in some fashion the author would appear to exclude one of his most helpful sources of support. I refer to the possibility of cerebral activities which are real motor processes as far as they go, but which may never come to any peripheral expression. While he admits (p. 326) that his view of laryngeal organization "is largely an assumption" and "the experimental evidence for this view is slight," he ignores cortical organization, about which certainly as much is known. This is not merely because cortical processes cannot readily be demonstrated by instrumentation, for he uses freely the supposed functions of ductless glands which are quite as inaccessible. We can account for this neglect only as an expression of a general neurophobia which permeates the work.

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Chapter x describes the physiology of muscular work, mental arithmetic, the effect of drugs on habit systems, the effect of climate and other factors on the curve of work, some factors affecting the acquisition of habits, and a few other related topics. The final chapter is devoted largely to an outline for the analytic study of personality, or the "reaction mass as a whole."

As a critical summary of the established facts of human behavior this book is cordially welcomed, and as a source-book it is sure to have a wide usefulness. How far it will prove adapted for use as a textbook in American colleges will, of course, be decided by the teachers of psychology themselves. The presentation is cast in a form so strongly suggestive of the special plea of an advocate as to tend to appeal unfavorably to those not already of the author's way of thinking. And this unfortunately applies to some who, like the reviewer, are thoroughly sympathetic with the program suggested at the beginning of this paragraph.

Psychology, "from the standpoint of a behaviorist"—just what is the scope of the science (to recur to our first question) as thus restricted? The reviewer must confess that a careful reading of the volume has failed to yield up to him the answer. From the introductory definition it may be inferred that attention is limited to human psychology: "Psychology is that division of natural science which takes human activity and conduct as its subject matter."

Whether a science of comparative psychology is possible and, whatever answer is given to this question, what may be the relation between the study of animal behavior and human psychology, are topics nowhere explicated. The treatment presented in this book, taken in connection with other recent writings by the author, gives the impression that the work is intended as a general introduction to the science broadly interpreted, and so it must be evaluated.

An eminent student of animal behavior has recently written an elementary textbook from the standpoint of "pure psychology," using only introspective data, in which the student is nowhere given a hint that "the mind has a body" or is in any way related to one. And now his equally eminent colleague in the establishment of the "American school" of animal behavior seems to feel constrained to apply a drastic antidote "from the standpoint of a behaviorist," using purely objective data, in which the student is nowhere permitted to recognize consciousness or to admit the need of that function.

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One wonders whether it is expedient to introduce the elementary student to the science of psychology from any special standpoint. In our better medical schools we object to the teaching of anatomy and physiology from the standpoint of the surgeon, the obstetrician, or any other specialist; we do desire that the student shall know the whole body as a working machine. Those who view their science thus artificially contracted must beware lest they in the end find themselves in possession of a pseudo-science or of disjointed fragments of many sciences.

Indeed in viewing psychology exclusively from the standpoint of radical behaviorism the disquieting fear continually arises that possibly the beholder may find himself in the unhappy position of the venerable Hebrew Lawgiver, who was permitted to look out from Nebo's lofty height over into the Promised Land, which, however, for his sins he was forbidden to enter.

C. JUDSON HERRICK.

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INTEGRATION OF MOVEMENTS IN LEARNING IN THE ALBINO RAT

A STUDY OF THE ADJUSTMENT OF AN ORGANISM TO AN ENVIRONMENT

JOHN LINCK ULRICH

III. THE INCLINED-PLANE PROBLEM

The extension of investigations similar to those made with the latch-box problem, to another, the inclined-plane problem, was necessary in order to substantiate the importance of all reflex mechanisms interacting in learning. In the latch-box problem, it was seen that a localized part of the rat's organization, the neck reflex movement, was used in interacting with other reflex parts. The sensory mechanism, that of extension and flexion of the limbs, that for the production of reflex excitability and other reflex parts was seen to condition in rats the production of the neck reflex movement. Because of the existence of a mechanism to be manipulated, but different from that present in the latch-box problem, another localized reflex movement interacting with other reflex parts will be used to solve the inclined-plane problem. Since the development of interaction of body parts is dependent upon the functional condition of the reflex extensor thrusts and that for the production of reflex excitability, these parts will again function. On the whole, however, the same physiological changes for learning will exist.

The inclined-plane problem, figure 3, consisted of a box cage similar to that of the latch-box but somewhat higher. It was 31 cm. square and 33 cm. high, and in the center of one side was a small door 11.5 by 12.5 cm. The inclined-plane, figure 4, was made of two pieces of sheet aluminum 15.5 by 6.5 cm. hinged together at one end. The upper inclined surface was covered with black wood fiber. The plane supported by a bronze spring made an angle approximately 44 degrees with the base. To the under surface of the plane was attached a freely moving

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lever. Electric contacts were placed on the longer arm of the lever and on the base of the plane. These points were connected in circuit with a solenoid coil, the armature of which held the door closed. The coil was operated by a house current of 110 volts with a rheostat in circuit to obtain the exact current needed. The plane was placed 12 cm. back of the problem

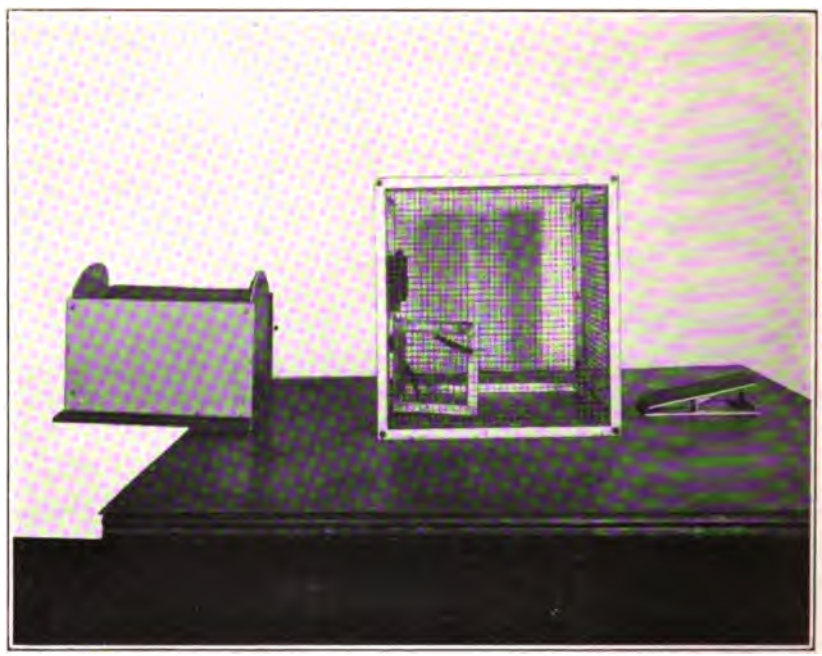

FIG. 3. INCLINED PLANE

box in direct line with the center of the door and the entrance to the wire hood which covered the problem.

The principle of the plane is that of a lever, the fulcrum being at one end of the base. The existence of another lever underneath the upper arm of the first makes the entire apparatus a double lever. With such an arrangement, slight pressure on the upper surface of the plane brought about a better electrical contact when the plane was plunged, and the movement of the plane downward could be accomplished through a greater dis-

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tance with less pressure than would be required if a single lever was present. The bronze spring supporting the arms of the plane and separating the electrical contact points was sufficiently elastic to keep these points 1.5 cm. apart. The exact degree of pressure needed to plunge such a plane was determined by experimentation.

The experimental procedure when investigating learning in the inclined-plane problem differed little from that adopted with the latch-box problem All rats were 40 days instead of 50 days old when fed for 2 days in the problem. One trial a day method was used. During this period of feeding, the plane was thrown back on its hinges, so that in the wanderings of the rats, the plane would not be plunged and the interaction of the body parts that would be partly established in consequence of the act, be less evident in the first trial. The relation of the first to succeeding trials is exceedingly important to observe, and the plunging of the plane for the first time must have the same effect upon rats as that of the raising of the latch of the latch-box problem.

In order to accomplish the solution of the problem, it is necessary that progression be directed from the entrance box to the back of the problem box and to the plane, the plane plunged, and then progression be directed to the front of the problem to the opened door. Direct progresion to the plane and then to the door after the plane is plunged may be regarded as two separate parts of directive integration One additional movement must be facilitated in learning the inclined-plane that is not used in the latch-box problem, namely, direct progression for some distance from the mechanism which opens the door of the problem box to the food. One difference in the results from those obtained from the latch-box problem must be considered.

During the first trial the behavior of the rat was carefully noted to see how far a sequence of events determines the character of this trial. This sequence is more readily observable when the functional condition of the rat's reflex mechanism is known. Of the few movements made, all are directed to the problem, to the door, or to the plane. The response to the

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plane is similar to a response to any foreign object that is placed in the living cage. All movements made indicate that manifold stimuli or the configuration of the problem are effective in producing excitations in the external senses which produce directive movements, and posture for progression to any part of the problem box is possible. At this time, it was noted that posture was essential for progression and in particular to any direction to perform a difficult movement. On the other hand, when the functional condition of the organism is not the best, slow or rapid progression, in short or long spurts and climbing seem to be the product of the functional undeveloped condition of the rat's reflex mechanisms rather than from definite sensory excitations. Numerous stimuli or the configuration of the problem increase the fundamental process of progression, and it is conducted in a variety of directions. In such cases, movements become directive when reflex excitability becomes hypernormal, but, if it is hyponormal, not until a cumulative effect of the performance of a number of movements develops interaction of body parts can progression occur in a definite direction. Movements in a definite direction are not exclusively produced by excitations in the external senses; but when posturing favoring progression in a definite direction is possible, then the external senses can produce movements to or from objects. Progression is directive when it leads to touching, to smelling, or to looking at any part of the problem box, such as the door or the plane. An increase in reflex excitability, or the appearance of timidity at this time are additional responses produced by the external senses. All these movements must produce an additional cumulative effect on the organism.

Since the degree of reflex excitability manifest by rats is so different, the cumulative effect produced by all responses must not be the same in all rats. It was seen in the investigation of learning in the latch-box problem that reflex excitability was the most favorable thing possessed by a rat for solving this problem; for only when reflex excitability was hypernormal, could a decided cumulative effect from all movements alone reach a point where responses were effectively produced. But, because

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of the ease with which the plane can be plunged, all rats, even those manifesting hyponormal reflex excitability, can solve the inclined plane problem. Consequently, different results and additional facts in relation to reflex excitability may be expected.

Considerable mention of the cumulative effect produced by all movements has been made to emphasize its importance, for in the inclined-plane problem this effect is significant in learning. It will be remembered that an interaction of body parts for the production of the neck reflex movement and those accessory to it appeared when this movement was first produced. It is apparent that plunging of the plane ought to produce the same effect in initiating interaction of body parts for learning the inclined-plane problem, but this effect does not appear in all rats at the same time. Interaction is readily observed to be initiated in rats when reflex excitability is manifest and an additional cumulative effect is brought about by the plane being plunged, for at this time sensory excitations aroused by the plane going down, produce responses which have a profound effect on all movements in succeeding trials. When, however, excitability is hyponormal, and no cumulative effect on tonic reflexes occur, the going down of the plane scarcely produces any additional cumulative effect on these reflexes and the initiating of interaction to learn the problem. Several successive plungings of the plane are required before learning is at all perceptible.

Preliminary work with the inclined plane problem revealed the fact that usually the extensor thrust of the fore limbs or some modified form of it is used to plunge the plane. In some instances the plane was ascended, but this method was not continued for more than a trial or two. The exclusive use of the extensor thrust of the fore thrust proved to be the method most effectively used. When the extensor thrust was not exclusively used, no one method was as a rule consistently employed throughout learning, but generally at the end of learning one method was adhered to.

Two conditions seemed to determine this inconstancy of method. Either the mechanism of the plane was at fault, too

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great a pressure was required to plunge it, or the employment of more than one method was due to the undeveloped condition of the reflex extensor thrusts. Additional observations pointed to the fact that the mechanism of the plane used in this preliminary work was at fault, and when an adequate plane was constructed, it was seen that in a measure the functional condition of the extensor thrusts determined the method used. The inadequacy of the plane used led to the construction of the plane seen in figure 4. This plane produced many more effective extensor thrusts than were produced with any other plane. The necessity of adjusting the working mechanism of every

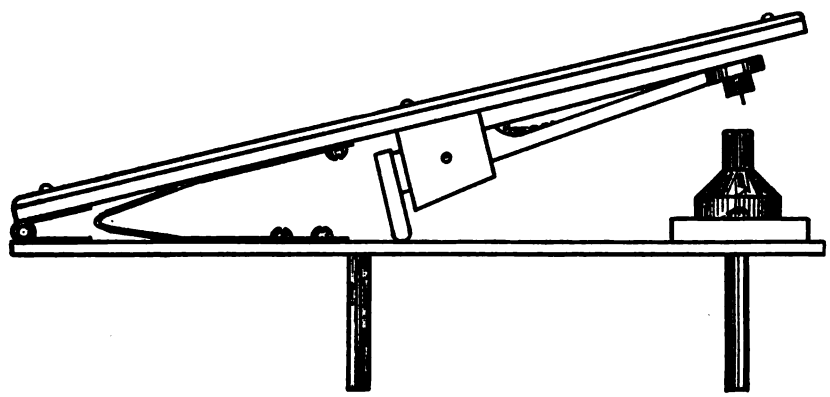

FIG. 4. THE PLANE

problem was previously seen to be of importance when investigating learning in the latch-box problem, when the height of the latch, and the depth of the notch in which the latch was placed, had to be determined before there could be produced the neck reflex movement adequate to raise the latch. With the inclined-plane problem, this adjustment of the mechanism of the problem was found to be even more essential, for if the plane did not descend readily, other methods, stepping on the plane, ascending it, or partly ascending it, occurred to plunge it. When the extensor thrust was not used, the weight of the body helped to bring down the plane. Consequently it was impossible to discover the most effective method. When the

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effective thrust was discovered, one or both fore limbs were used to plunge the plane, and no part of the body, with the exception of the head of the rat, was at any time directly over the plane. The weight of the body was not an additional aid in bringing the plane down. In all previous work in animal behavior, the adjustment of the mechanism of the problem to produce the most effective response, and an interest in the nature of a definite effective response have been neglected. In the past the thing of the greatest interest seemed to be the production of a "successful movement" and its "retention."

When an adequate plane was constructed, an effective extensor thrust was, in only a few instances employed for successive trials. It is scarcely necessary to recall the fact that in most rats these extensor thrusts are functionally undeveloped or that an unequal antagonistic action of the extension and flexion of the limbs exists, and only in rare instances are the thrusts developed or an equal antagonistic action of the limbs prevails. Fluctuations in the extensor thrusts occur in every case when an unequal antagonistic action is present. Of the 250 rats actually experimented with in the inclined-plane problem, only two rats were obtained in which the thrusts were fairly well developed. All other rats when tested, evoked a left or right ipsilateral fore reflex thrust, or the extensor tone of one fore ipsilateral thrust was stronger than the other when the head of the rat was passively bent to one side of the midline of the body, or when the head was dorsally extended 45 degrees of this line. The hind reflex thrusts were strongly evoked backward in a fixed position or rhythmic extension and flexion of them occurred. In some rats, the fore reflex extensor thrusts could not be evoked, and in others all reflex thrusts were absent.

For the present, it is convenient to consider only the method used on the first trial to plunge the plane, though a later method must at this time be incidentally mentioned to give significance to the one used on the first trial. The use of more than one method makes an investigation of learning rather difficult. Frequently many rats were used to clear up or substantiate one particular observation.

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In the investigation of learning in the latch-box problem, it was discovered that when reflex excitability was manifest in rats and there existed bilateral symmetrical development of the reflex extensor thrusts, the production of the neck reflex movement to raise the latch was without difficulty produced. Such conditions existing in rats proved to be the best for the production of the extensor thrust to plunge the plane. The extensor thrust was used on the first trial, and continued to be employed throughout learning. Two rats were obtained that possessed fairly well developed reflex thrusts, only a slight decrease in extensor tone of a limb or a disappearance at times of one fore ipsilateral extensor thrust occurred. With the use of the extensor thrusts of the fore limbs on the first trial and throughout learning, conditions resemble those when the neck reflex is effectively produced on the first trial and during learning. Posturing is necessary to produce the extensor thrust, as it is to produce the neck reflex movement. Posture is greatly increased on the first trial when the rat's snout touches the latch or slightly moves it, and is once more increased when the fore limbs touch the upper part of the plane and it gives way. Immediately on posturing, the extensor thrust of one or both fore limbs is produced to plunge the plane. The rat's body is slightly crouched before the plane is neared. The effective performance of every movement is quite apparent.

In all other rats, in the absence of functionally developed reflex extensor thrusts, posturing before the plane does not take place and progression is continued over the plane as it is neared, or a modified extensor thrust is produced. When progression is continued over the plane, the plane is ascended or stepped on, and when a modified extensor thrust is produced, the fore limbs and the weight of the fore part of the body bring the plane down. Sometimes when the modified thrust is used, the hind quarters may lower in the act of posturing. This is but a feeble attempt to attain posture. The functional development of the extensor thrusts of the rat's limbs determines the method used, and as was the case with the latch-box problem, all degrees of effectiveness to operate the mechanism of the problem are observable.

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The attainment of posture with the effective production of the neck reflex movement was observed to be facilitated in rats manifesting reflex excitability. In the case of the inclined-plane problem all rats can solve it, and consequently the results obtained are quite different. Posture can not be produced when the reflex thrusts are undeveloped even when reflex excitability is hypernormal. Some effect on the organism is evident, however, when excitability is manifest. This effect is shown in the rapidity and constancy with which a response, or even a method to plunge the plane occurs, and the contrary is the case when reflex excitability in rats is hyponormal. It is conceivable that rats manifesting hyponormal reflex excitability before the plane was plunged, would not likely show any increased excitability thereafter. Little or no disturbance in the movements of rats is observable. When the plane is plunged, progression from the plane is as slow as when ascending or walking over the plane often takes place. On the other hand, an evident increase in excitability is manifest in rats, in which reflex excitability is hypernormal. A parallel to this was seen with the latch-box problem when an evident increase in excitability was noticeable in rats after the latch was raised, and taking of food did not occur until reflex excitability had subsided. When the plane was plunged, progression from the plane was very rapid and continued frequently for some distance. Sometimes descent from the plane or turning from it was attended by gazing at it or smelling it. In one or two instances progression was rapid to the front of the problem and then slowly proceeded to the open door and to the food.

If our conclusions in regard to the importance of the existence of reflex excitability are correct, then the appearance of interaction of body parts for learning will in some rats appear before the plane is plunged and in others not until the plane is plunged, or when reflex excitability is hyponormal, not until several trials and then only after the plane is plunged. In the last instance either reflex excitability or interaction is developed. In order that the appearance of interaction of body parts be shown to be developed at different periods of learning in accord-

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ance with the degree of reflex excitability manifest, two kinds of tables are presented. Table 10 gives the record of 24 rats. In this table will be found for each rat the trial when direct progression to the plane from the entrance box occurs for the first time, and the trial when progression is direct to the door after the plane is plunged. In addition the trial in which direc-

TABLE 10

NUMBER	TO PLANE	TO DOOR	DIRECTIVE INTEGRATION FACILITATED	REFLEX THRUSTS	REFLEX EXCITABILITY
1	4	5	8	All fluctuated	Hypernormal
2	10	3	11	Right fore weak	Hyponormal
3	10	6	11	Left fore weak	Hyponormal
4	12	6	13	All fluctuated	Hyponormal
5	9	6	9	All fluctuated	Hyponormal
6	8	8	8	Left fore	Hypernormal
7	6	2	8	Left fore or none	Hyponormal
8	5	8	13	None	Hypernormal
9	3	5	6	Right fore or none	Hypernormal
10	4	5	12	Right fore	Hypernormal
11	6	3	6	Left fore or none	Hyponormal
12	5	6	12	Right fore or none	Hypernormal
13	5	7	7	All fluctuated	Timid
14	18	4	18	None	Hyponormal
15	5	4	5	All fluctuated	Hyponormal
16	7	7	7	All fluctuated	Hypernormal
17	22	15	22	Right fore	Hyponormal
18	13	8	13	All fluctuated	Hyponormal
19	16	12	21	All fluctuated	Hyponormal
20	2	2	5	All fluctuated slightly	Hypernormal
21	1	2	13	All fluctuated slightly	Hypernormal
22	5	7	13	All fluctuated	Hypernormal
23	8	5	11	All fluctuated	Hyponormal
24	8	7	8	Right fore	Hyponormal

tive integration is facilitated is also stated. The degree of reflex excitability existing in the first trial and a general statement of the functional condition of the reflex thrust as observed throughout learning is also given. Tables 11, 12, 13, 14, and 15 give of 5 rats for 60 trials the reaction time for each part of directive integration, the total time to solve the problem, the functional condition of the reflex extensor thrusts of all limbs from trial to

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trial, and the degree of reflex excitability manifest. The development of directive integration is also indicated. The reaction times for each trial were taken with a stop watch with split seconds.

The first absolute indication of the appearance of interaction of body parts in learning is when directive integration is about to be established. Because of the different times in which an increase in reflex excitability occurs in rats, interaction for direct progression to or from the plane takes place in accordance with this increase. To some extent direct progression in either direction is dependent upon the functional condition of the reflex thrust, but mainly upon the degree of reflex excitability manifest by rats. Table 10 shows that directive integration appears very early in rats with functionally developed reflex thrusts and when, on the first trial, reflex excitability is hypernormal. When these mechanisms are functionally undeveloped, direct progression to the door and to the plane are not facilitated at the same time. When reflex excitability is hypernormal, that part of directive integration, direct progression to the plane appears first, before direct progression to the door or before both parts are facilitated at the same time. On the other hand when reflex excitability is hyponormal, that part of directive integration, direct progression to the door is facilitated first. Efficient movements result from the increase, or the production of reflex excitability by the plunging of the plane. When reflex excitability is strikingly hyponormal, it is necessary that plunging of the plane take place for several trials before reflex excitability is increased, and direct progression to the door can be facilitated. In some instances, rats 15 and 24, table 10, when excitability is scarcely hyponormal and the reflex extensor thrust undeveloped, direct progression to the door may occur only a trial before the appearance of direct progression to the plane.

From the results presented in table 10, it is seen that the development of interaction to facilitate the appearance of directive integration is generally a gradual one. The first appearance of either one or both parts of directive integration does not indicate that it is completely facilitated. Often many trials are

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TABLE 11

NUMBER	TIME			DIRECTIVE INTEGRATION		DIRECTION TO PLANE	REFLEX THRUSTS			REFLEX EXCITABILITY	METHOD
	To plane	To door	Total	To plane	To door		Fore	45 degrees	Hind		
1	6	10	16			Right	Both	Right	Strong	Hypernormal	Thrust
2	2	4	6	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
3	30	5	35	Direct	Direct	Right	Both	Both	Strong	Hypernormal	Thrust
4	6	3	9	Direct	Direct	Left	Both	Both	Strong	Hypernormal	Thrust*
5	363	19	382	Direct		Left	Both	Right	Strong	Hypernormal	Thrust
6	85	23	108			Right	Both	Right	Strong	Hypernormal	Thrust
7	6	2	8	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
8	158	10	168	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust
9	12	8	20	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust
10	190	4	194	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust
11	11	1	12	Direct	Direct	Left	Both	Right	Strong	Hypernormal	Thrust
12	78	6	84	Direct	Direct	Left	Both	Right	Strong	Hypernormal	Thrust
13	1	1	2	Direct	Direct	Left	Both	Right	Strong	Hypernormal	Thrust*
14	13	2	15	Direct	Direct	Left	Both	Right	Strong	Hypernormal	Thrust
15	2	1	3	Direct	Direct	Left	Both	Right	Strong	Hypernormal	Thrust*
16	13	1	14	Direct	Direct	Left	Both	Right	Strong	Hypernormal	Thrust
17	1	2	3	Direct	Direct	Left	Both	Right	Strong	Hypernormal	Thrust*
18	25	1	26	Direct	Direct	Left	Both	Right	Strong	Hypernormal	Thrust
19	68	2	70	Direct	Direct	Left	Both	Right	Strong	Hypernormal	Thrust
20	11	2	13	Direct	Direct	Left	Both	Right	Strong	Hypernormal	Thrust
21	24	2	26	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust
22	2	2	4	Direct	Direct	Left	Both	Right	Strong	Hypernormal	Thrust*
23	2	1	3	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
24	2	1	3	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
25	2	1	3	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
26	2	2	4	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
27	2	1	3	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
28	4	1	5	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
29	2	1	4	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*

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30	1	1	2	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
31	2	1	3	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
32	2	2	4	Direct	Direct	Left	Both	Right	Strong	Hypernormal	Thrust*
33	2	1	3	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
34	5	1	6	Direct	Direct	Right	Both	R. str.	Strong	Hypernormal	Thrust
35	1	1	2	Direct	Direct	Right	Both	R. str.	Strong	Hypernormal	Thrust*
36	1	1	2	Direct	Direct	Right	Both	R. str.	Strong	Hypernormal	Thrust*
37	1	1	2	Direct	Direct	Right	Both	R. str.	Strong	Hypernormal	Thrust*
38	1	1	2	Direct	Direct	Right	Both	R. str.	Strong	Hypernormal	Thrust*
39	1	1	2	Direct	Direct	Right	Both	R. str.	Strong	Hypernormal	Thrust*
40	1	1	2	Direct	Direct	Right	Both	R. str.	Strong	Hypernormal	Thrust*
41	1	1	2	Direct	Direct	Right	R. str.	R. str.	Strong	Hypernormal	Thrust*
42	1	1	2	Direct	Direct	Right	R. str.	None	Strong	Hypernormal	Thrust*
43	1	1	2	Direct	Direct	Right	R. str.	Right	Strong	Hypernormal	Thrust*
44	1	1	2	Direct	Direct	Right	R. str.	None	Strong	Hypernormal	Thrust*
45	4	1	5	Direct	Direct	Right	R. str.	None	Strong	Hypernormal	Thrust*
46	1	1	2	Direct	Direct	Right	R. str.	Right	Strong	Hypernormal	Thrust*
47	3	1	4	Direct	Direct	Right	R. stf.	Right	Strong	Hypernormal	Thrust*
48	1	1	2	Direct	Direct	Right	R. str.	Right	Strong	Hypernormal	Thrust*
49	2	1	3	Direct	Direct	Right	R. str.	Right	Strong	Hypernormal	Thrust*
50	1	1	2	Direct	Direct	Right	R. str.	R. str.	Strong	Hypernormal	Thrust*
51	1	1	2	Direct	Direct	Right	R. str.	R. str.	Strong	Hypernormal	Thrust*
52	1	1	2	Direct	Direct	Right	R. str.	R. str.	Strong	Hypernormal	Thrust*
53	1	1	2	Direct	Direct	Right	R. str.	None	Strong	Hypernormal	Thrust*
54	1	1	2	Direct	Direct	Right	R. str.	None	Strong	Hypernormal	Thrust*
55	3	1	4	Direct	Direct	Right	Right	R. str.	Strong	Hypernormal	Thrust
56	2	2	4	Direct	Direct	Right	Both	R. str.	Strong	Hypernormal	Thrust*
57	1	1	2	Direct	Direct	Right	R. str.	R. str.	Strong	Hypernormal	Thrust*
58	1	1	2	Direct	Direct	Right	R str.	R. str.	Strong	Hypernormal	Thrust*
59	1	1	2	Direct	Direct	Right	R. str.	R. str.	Strong	Hypernormal	Thrust*
60	1	1	2	Direct	Direct	Right	R. str.	R. str.	Strong	Hypernormal	Thrust*

R. str. = right stronger.
* Perfect record.

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TABLE 12

NUMBER	TIME			DIRECTIVE INTEGRATION		DIRECTION TO PLANE	REFLEX THRUSTS			REFLEX EXCITABILITY	METHOD
	To plane	To door	Total	To plane	To door		Fore	45 degrees	Hind		
1	68	13	81				Both	Right	Strong	Normal	Thrust
2	1561	14	1575				Both	None	Strong	Timid	Thrust
3	273	57	330			Left	R. str.	None	Strong	Timid	On
4	100	8	108			Left	Both	None	Strong	Timid	Thrust
5	1464	2	1466			Left	Both	Both	Strong	Timid	Thrust
6	80	2	82			Left	Both	Both	Strong	Timid	Thrust
7	50	3	53	Direct	Direct	Right	Both	None	Strong	Timid	Thrust
8	27	1	28	Direct	Direct	Right	Both	None	Strong	Timid	Thrust
9	186	1	187	Direct	Direct	Right	Both	None	Strong	Timid	Thrust
10	20	1	21	Direct	Direct	Right	Both	None	Strong	Hypernormal	Thrust
11	6	1	7	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
12	3	1	4	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
13	4	1	5	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
14	5	1	6	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
15	6	1	7	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
16	4	1	5	Direct	Direct	Right	Right	Right	Strong	Hypernormal	Thrust*
17	3	13	16	Direct		Right	Right	Right	Strong	Hypernormal	Thrust
18	3	1	4	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
19	4	1	5	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
20	2	2	4	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
21	2	2	4	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
22	2	2	4	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
23	3	2	5	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
24	2	1	3	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
25	2	1	3	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
26	4	1	5	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
27	2	1	3	Direct	Direct	Right	Right	Both wk.	Strong	Hypernormal	Thrust*
28	2	1	3	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
29	2	1	3	Direct	Direct	Right	Both	L. wk.	Strong	Hypernormal	Thrust*
30	2	1	3	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*

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31	2	1	3	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
32	2	1	3	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
33	2	1	3	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
34	2	1	3	Direct	Direct	Right	Both	Left	Strong	Hypernormal	Thrust*
35	2	1	3	Direct	Direct	Right	Both	Left	Strong	Hypernormal	Thrust*
36	2	1	3	Direct	Direct	Right	Both	L. str.	Strong	Hypernormal	Thrust*
37	1	1	2	Direct	Direct	Right	Both	L. str.	Strong	Hypernormal	Thrust*
38	1	2	3	Direct	Direct	Right	R. str.	R. wk.	Strong	Hypernormal	Thrust*
39	1	1	2	Direct	Direct	Right	R. str.	Both wk.	Strong	Hypernormal	Thrust*
40	2	1	3	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
41	3	1	4	Direct	Direct	Right	Right	None	Strong	Hypernormal	Thrust*
42	1	1	2	Direct	Direct	Right	Both	None	Strong	Hypernormal	Thrust*
43	3	1	4	Direct	Direct	Right	Right	R. wk.	Strong	Hypernormal	Thrust*
44	2	2	4	Direct	Direct	Right	Right	Left wk.	Strong	Hypernormal	Thrust*
45	2	2	4	Direct	Direct	Right	Both	Both	Strong	Hypernormal	Thrust*
46	3	1	4	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
47	2	1	3	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
48	9	1	10	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
49	2	2	4	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
50	2	1	3	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
51	2	1	3	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
52	1	1	2	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
53	14	4	18	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust
54	2	1	3	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
55	2	1	3	Direct	Direct	Right	Both	Right	Strong	Hypernormal	Thrust*
56	2	1	3	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
57	2	1	3	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
58	2	1	3	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*
59	14	1	15	Direct	Direct	Right	Right and cont. Left	Both wk.	Strong	Hypernormal	Thrust
60	3	1	4	Direct	Direct	Right	Both	Both wk.	Strong	Hypernormal	Thrust*

R. or L. str. = right or left stronger; right or left wk., = weak.

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TABLE 13

NUMBER	TIME			DIRECTIVE INTEGRATION		DIRECTION TO PLANE	REFLEX THRUST			REFLEX EXCITABILITY	METHOD
	To plane	To door	Total	Plane	Door		Fore	45 degrees	Hind		
1	206	39	245		Direct		R. wk.	None	Weak	Hyponormal	On
2	80	2	82		Direct		None	None	Weak	Hyponormal	On
3	36	2	38		Direct		None	None	Weak	Hyponormal	Step
4	24	2	26		Direct		None	None	Weak	Hypernormal	Step
5	25	2	28		Direct	Left	None	None	Weak	Hypernormal	M. T.
6	26	2	28	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
7	12	4	16		Direct	Right	None	None	Weak	Hypernormal	M. T.
8	19	2	21	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
9	1	2	3	Direct	Direct	Right	L. wk.	None	Weak	Hypernormal	M. T.
10	5	2	7	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.*
11	3	2	5	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
12	3	1	4	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
13	7	1	8	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
14	4	1	5	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
15	6	1	7	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
16	3	2	5	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
17	3	2	5	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
18	8	1	9	Direct	Direct	Right	L. wk.	None	Weak	Hypernormal	Step
19	3	2	5	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
20	3	2	5	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
21	3	1	4	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
22	3	1	4	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
23	2	2	4	Direct	Direct	Right	None	None	Weak	Hypernormal	Step*
24	13	1	14	Direct	Direct	Right	None	None	Weak	Hypernormal	On
25	23	2	25	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
26	10	2	12	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
27	3	2	5	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
28	1	1	2	Direct	Direct	Right	None	None	Weak	Hypernormal	Step*
29	3	1	4	Direct	Direct	Right	None	None	Weak	Hypernormal	Step

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30	3	1	4	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
31	3	1	4	Direct	Direct	Right	None	None	None	Hypernormal	Step
32	3	1	4	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
33	4	2	6	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
34	11	2	13	Direct	Direct	Right	None	None	None	Hypernormal	Step
35	3	2	5	Direct	Direct	Right	None	None	None	Hypernormal	Step
36	3	1	4	Direct	Direct	Right	None	None	None	Hypernormal	Step
37	3	1	4	Direct	Direct	Right	None	None	None	Hypernormal	Step
38	2	1	3	Direct	Direct	Right	None	None	None	Hypernormal	Step
39	8	1	9	Direct	Direct	Right	None	None	None	Hypernormal	Step
40	7	1	8	Direct	Direct	Right	None	None	None	Hypernormal	Step
41	3	1	4	Direct	Direct	Right	None	None	None	Hypernormal	Step
42	10	1	11	Direct	Direct	Right	None	None	None	Hypernormal	Step
43	3	1	4	Direct	Direct	Right	None	None	None	Hypernormal	Step
44	2	1	3	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
45	3	1	4	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
46	4	1	5	Direct	Direct	Right	None	None	None	Hypernormal	On
47	3	2	5	Direct	Direct	Right	None	None	None	Hypernormal	Step
48	3	1	4	Direct	Direct	Right	None	None	None	Hypernormal	Step
49	5	1	6	Direct	Direct	Right	None	None	None	Hypernormal	Step
50	6	1	7	Direct	Direct	Right	None	None	None	Hypernormal	Step
51	6	1	7	Direct	Direct	Right	None	None	None	Hypernormal	Step
52	23	1	24	Direct	Direct	Right	None	None	None	Hypernormal	On
53	22	2	24	Direct	Direct	Right	None	None	None	Hypernormal	Step
54	2	2	4	Direct	Direct	Right	None	None	None	Hypernormal	On
55	9	2	11		Direct	Right	None	None	None	Hyponormal	Step
56	3	1	4	Direct	Direct	Right	None	None	None	Hypernormal	Step
57	3	1	4	Direct	Direct	Right	None	None	None	Hypernormal	On
58	5	1	6	Direct	Direct	Right	None	None	None	Hypernormal	Step
59	11	1	12	Direct	Direct	Right	None	None	None	Hypernormal	Step
60	6	1	7	Direct	Direct	Right	None	None	None	Hypernormal	Step

M. T. = Modified Thrust.

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TABLE 14

NUMBER	TIME			DIRECTIVE INTEGRATION		DIRECTION TO PLANE	REFLEX THRUSTS			REFLEX EXCITABILITY	METHOD
	To plane	To door	Total	To plane	To door		Fore	45 degrees	Hind		
1	70	33	103				Both	R. wk.	Strong	Hyponormal	On
2	71	37	108			Left	R. str.	None	Strong	Hyponormal	On
3	40	4	44			Left	R. str.	R. wk.	Strong	Hyponormal	On
4	34	2	36		Direct	Right	R. str.	None	Strong	Hyponormal	On
5	104	2	106	Direct	Direct	Right	R. str.	None	Weak	Hypernormal	On
6	10	1	11	Direct		Left	Right	None	Weak	Hypernormal	M. T.
7	85	2	87	Direct	Direct	Right	Both wk.	None	Weak	Hypernormal	M. T.
8	14	1	15	Direct	Direct	Left	Both wk.	None	Weak	Hypernormal	M. T.
9	6	1	7	Direct	Direct	Left	R. wk.	None	Weak	Hypernormal	M. T.
10	4	1	5	Direct	Direct	Left	R. wk.	None	Weak	Hypernormal	M .
11	7	1	8	Direct	Direct	Left	R. wk.	None	Weak	Hypernormal	[illegible]
12	4	2	6	Direct	Direct	Left	R. wk.	None	Weak	Hypernormal	M. T.
13	2	1	3	Direct	Direct	Left	R. wk.	None	Weak	Hypernormal	M. T.
14	4	1	5	Direct	Direct	Left	R. wk.	R. wk.	Weak	Hypernormal	On
15	4	1	5	Direct	Direct	Left	Both wk.	None	Strong	Hypernormal	M. T.
16	2	1	3	Direct	Direct	Left	Both wk.	R. wk.	Strong	Hypernormal	Step
17	1	1	2	Direct	Direct	Left	Both wk.	R. wk.	Strong	Hypernormal	Step
18	3	1	4	Direct	Direct	Left	Both wk.	R. wk.	Strong	Hypernormal	Step
19	1	1	2	Direct	Direct	Left	Both wk.	R. wk.	Strong	Hypernormal	M. T.
20	3	1	4	Direct	Direct	Left	R. str.	R. wk.	Strong	Hypernormal	Step*
21	1	1	2	Direct	Direct	Left	R. str.	R. wk.	Strong	Hypernormal	Step
22	1	1	2	Direct	Direct	Left	R. str.	R. wk.	Strong	Hypernormal	Step*
23	1	1	2	Direct	Direct	Left	R. str.	R. wk.	Strong	Hypernormal	Step*
24	1	1	2	Direct	Direct	Left	R. str.	R. wk.	Strong	Hypernormal	Step*
25	1	1	2	Direct	Direct	Left	R. str.	R. wk.	Strong	Hypernormal	Step*
26	1	1	2	Direct	Direct	Left	R. str.	R. wk.	Strong	Hypernormal	Step*
27	4	1	5	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step
28	2	1	3	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step*
29	3	1	4	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step

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30	3	1	4	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step
31	1	1	2	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step*
32	1	1	2	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step*
33	1	1	2	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step*
34	1	1	2	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step*
35	1	1	2	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step*
36	3	1	4	Direct	Direct	Left	R. str.	R. wk.	Strong	Hypernormal	Step
37	9	1	10	Direct	Direct	Left	R. wk.	R. wk.	Weak	Hypernormal	Step
38	1	1	2	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step*
39	3	1	4	Direct	Direct	Left	R. str.	R. str.	Strong	Hypernormal	Step
40	3	1	4	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step
41	4	1	5	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step
42	3	1	4	Direct	Direct	Left	Both wk.	R. wk.	Strong	Hypernormal	Step
43	3	1	4	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step
44	15	1	16	Direct	Direct	Left	Both wk.	R. wk.	Strong	Hypernormal	Step
45	6	1	7	Direct	Direct	Left	Both wk.	R. wk.	Strong	Hypernormal	Step
46	6	1	7	Direct	Direct	Left	Both wk.	R. wk.	Strong	Hypernormal	Step*
47	3	1	4	Direct	Direct	Left	None	R. wk.	Strong	Hypernormal	Step
48	4	1	5	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step
49	7	1	8	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step
50	7	1	8	Direct	Direct	Left	R. wk.	R. wk.	Srong	Hypernormal	Step
51	3	1	4	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step
52	4	1	5	Direct	Direct	Left	R. wk.	R. str.	Strong	Hypernormal	Step
53	3	2	5	Direct	Direct	Left	R. str.	R. str.	Strong	Hypernormal	Step
54	3	1	4	Direct	Direct	Left	R. str.	R. wk.	Strong	Hypernormal	Step
55	3	1	4	Direct	Direct	Left	R. str.	R. wk.	Strong	Hypernormal	Step
56	8	3	11	Direct	Direct	Left	R. str.	R. wk.	Strong	Hypernormal	Step
57	3	1	4	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step
58	3	1	4	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step
59	4	1	5	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step
60	8	3	11	Direct	Direct	Left	R. wk.	R. wk.	Strong	Hypernormal	Step

M. T. = Modified Thrust.

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TABLE 15

NUMBER	TIME			DIRECTIVE INTEGRATION		DIRECTION TO PLANE	REFLEX THRUSTS			REFLEX EXCITABILITY	METHOD
	To plane	To door	Total	To plane	To door		Fore	45 degrees	Hind		
1	206	35	241			Right	Both	None	Strong	Hyponormal	On
2	400	15	415			Right	None	None	Weak	Hyponormal	M. T.
3	1792	143	1935			Right	None	None	None	Hyponormal	On
4	1320	34	1354			Right	R. wk.	None	None	Hyponormal	On
5						Right	None	None	None	Hyponormal	
6						Right	R. wk.	None	None	Hyponormal	
7	660	20	680			Right	R. wk.	None	None	Hyponormal	M. T.
8						Right	R. wk.	None	None	Hyponormal	
9						Right	R. wk.	None	None	Hyponormal	
10	222	30	252			Right	Both wk.	None	None	Hyponormal	M. T.
11	72	10	82			Right	Left	None	None	Hyponormal	M. T.
12	448	25	473			Right	Left	None	None	Hyponormal	M. T.
13	161	2	163			Right	R. wk.	None	None	Hyponormal	M. T.
14	409	7	416			Right	R. wk.	None	None	Hyponormal	M. T.
15	154	3	157		Direct	Left	R. wk.	None	None	Hyponormal	M. T.
16	8	8	16			Right	R. wk.	None	None	Hyponormal	M. T.
17	10	6	16			Right	R. wk.	None	None	Hyponormal	M. T.
18	167	7	174			Left	R. wk.	None	None	Hyponormal	M. T.
19	3	5	8	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
20	12	2	14	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
21	38	2	40		Direct	Left	R. wk.	None	None	Hypernormal	M. T.
22	4	2	6	Direct	Direct	Right	R. wk.	None	None	Hypernormal	M. T.
23	28	2	30	Direct	Direct	Left	None	None	None	Hypernormal	M. T.
24	10	1	11	Direct	Direct	Left	None	None	None	Hypernormal	M. T.
25	11	2	13	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
26	9	2	11	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
27	5	2	7	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
28	4	2	6	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
29	68	12	80		Direct	Left	R. wk.	None	None	Hyponormal	M. T.

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30	5	1	6	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
31	11	2	13	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
32	5	1	6	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
33	4	1	5	Direct	Direct	Left	R. wk.	None	None	Hypernormal	Step
34	10	1	11	Direct	Direct	Left	None	None	None	Hypernormal	M. T.
35	13	1	14	Direct	Direct	Left	None	None	None	Hypernormal	M. T.
36	2	1	3	Direct	Direct	Left	None	None	None	Hypernormal	M. T.
37	3	1	4	Direct	Direct	Left	None	None	None	Hypernormal	M. T.
38	1	2	3	Direct	Direct	Left	None	None	None	Hypernormal	M. T.
39	5	1	6	Direct	Direct	Left	None	None	None	Hypernormal	M. T.
40	4	1	5	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
41	13	2	15	Direct	Direct	Left	Both wk.	None	None	Hypernormal	Step
42	13	2	15	Direct	Direct	Left	R. wk.	None	None	Hypernormal	Step
43	5	1	6	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
44	5	1	6	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
45	27	30	57			Left	R. wk.	None	None	Hyponormal	On
46	5	1	6	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.*
47	50	1	51		Direct	Left	R. wk.	None	None	Hyponormal	On
48	9	2	11	Direct	Direct	Left	R. wk.	None	None	Hypernormal	On
49	5	1	6	Direct	Direct	Left	R. wk.	None	None	Hypernormal	On
50	4	2	6	Direct	Direct	Left	None	None	None	Hypernormal	On
51	19	1	20	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
52	8	1	9	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
53	33	1	34	Direct	Direct	Left	None	None	None	Hyponormal	M. T.
54	19	1	20		Direct	Left	None	None	None	Hyponormal	M. T.
55	5	2	7	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
56	4	1	5		Direct	Left	R. wk.	None	None	Hypernormal	M. T.
57	4	2	6		Direct	Left	R. wk.	None	None	Hypernormal	M. T.
58	4	2	6	Direct	Direct	Left	R. wk.	None	None	Hypernormal	M. T.
59	5	1	6	Direct	Direct	Left	Both wk.	None	None	Hypernormal	M. T.
60	6	2	8	Direct	Direct	Left	Both wk.	None	None	Hypernormal	M. T.

M. T. = Modified Thrust.

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required before interaction leading to its facilitation is developed. This gradual development is seen best from the results presented in tables 11 to 15. The first of these tables gives the record of a rat that possessed fairly well developed reflex extensor thrusts of all limbs and manifested hypernormal reflex excitability. The reaction time for the first trial, it will be noted, is very short and that of the second trial still shorter. Moroever the second trial is perfect. There is, because of the good functional condition of the rat, no necessity for a prolonged development of an interaction of body parts to facilitate responses. The behavior of the rat on the first trial showed a high degree of efficiency, for, when this rat was admitted to the problem, progression was rather slow and slightly diverted to the plane, the plane was touched by the fore paws, slight posture was attained, the extensor thrust was produced, then progression was rapid to the front of the problem and slow through the opened door. It may be argued that, on the first trial, progression could have been in any other direction than slightly diverted to the plane, and to the opened door after the plane was plunged, yet this does not preclude the fact that the functional mechanisms of the rat are fairly well developed.

Several more trials are required before directive integration is facilitated. Up to the seventh trial, progression is either to the right or to the left of the problem to the plane, and repeated advances to the plane occur. In this trial directive integration is facilitated. Occasionally, throughout learning, the direction of progression to the plane changes, but disruptions in directive integration and an increased difficulty to produce the extensor thrust is not observed to be present. The reason for the slow development of directive integration and the change in direction of progression to the plane, appears to be due to slight fluctuations in the extensor thrusts of the fore limbs when the rat's head was passively bent to the right or to the left of the midline of the body. The results of other rats presently to be considered, established the statement that fluctuations in an ipsilateral fore reflex thrust changes the direction of progression to the plane. When directive integration is facilitated, many

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consecutive perfect records are made during 60 trials. These are indicated in the table by an asterisk.

The record of another rat with fairly well developed reflex mechanistic parts is presented in table 12. Fluctuations in the reflex thrust occur rather more frequently than is shown in the record of the previous rat. Reflex excitability is hypernormal in the first two trials. Timidity is generally manifest for several trials. Directive integration does not appear until the seventh trial. The first perfect record occurs in the eleventh trial. The extensor thrust is used throughout the 60 trials given. Of all the rats used with the inclined-plane problem, more consecutive perfect records were obtained from this rat than all other rats, even exceeding the preceding rat's record. The functional condition of the preceding rat is, however, better than this one, for perfect records appeared sooner, in the second trial with the first rat, and not until the eleventh trial with the second rat.

The results obtained from these 2 rats point to a rather exceptional physiological efficiency for learning the inclined-plane problem. The extensor thrust was used throughout learning, and now it will be seen that the occurrence of many consecutive perfect records is dependent upon the attainment of posture before the plane and the production of the extensor thrust. Because of the inability to attain posture, other methods to plunge the plane became compulsory. Tables 13, 14, and 15 show a great difference in the effective performance of all responses and the number of trials, as well as the number of consecutive perfect records made. The rats whose records are presented in these tables possessed functionally undeveloped reflex extensor thrusts, and reflex excitability was hyponormal. Accordingly, that part of directive integration, progression to the door after the plane is plunged, occurred first before direct progression to the plane. Table 13 shows that direct progression to the door appeared in the second trial, to the plane in the sixth and directive integration was established in the eighth trial; in table 14 to the door in the fourth trial, to the plane in the fifth, and directive integration was facilitated in the seventh

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trial; in table 15 to the door in the fifteenth trial, to the plane in the nineteenth trial, and directive integration was not completely facilitated until the twenty-first trial. For some definite reason, different parts of directive integration are not facilitated at the same time, and its complete appearance is greatly delayed.

Directive integration is not established or facilitated until speed is gained, and this soon occurs when reflex excitability is hypernormal. Before speed is gained, movements are undirected, and no definite movements are constantly performed. The records of rats presented in the last three tables reveal the inconstancy of all responses, until reflex excitability becomes hypernormal, and the extensor tone of all integrated reflexes for posture to produce movements is facilitated. Wandering movements, progression either to the right or to the left of the problem to the plane and repeated advances to the plane, occur until movements are directive. Fluctuations in the reflex thrusts frequently become less, which means that progression direct to the plane or to the door is easily facilitated. That which requires a long time to accomplish in rats that manifest hyponormal reflex excitability and have undeveloped reflex thrusts, is present almost at the outset in rats with functionally developed reflex mechanisms.

When movements are more regularly directive then one method is generally used to plunge the plane. Either the plane is stepped on during rapid progression around the problem, or the modified thrust is used to bring the plane down. Not one of these methods is as efficient as the extensor thrust, for out of 60 trials very few consecutive perfect records are obtained, and at times only isolated perfect records are made. Table 13 shows only 3 perfect records and table 15, only 2. The employment of one constant method does not increase the number of perfect records, and this fact indicates that excitations in the external senses have definite limitations. Only after training can such excitations produce some effective responses.

The indefiniteness or the slowness with which some responses appear to be produced when reflex excitability is hyponormal in

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rats when learning the inclined-plane problem, has led many to conclude that the plunging of the plane in the first few trials is accidental. The plane may for 3 or 4 trials be ascended or stepped on without any evidence of a quickening of responses. Such a view of the accidental character of the early trials is quite in harmony with the one that all undirected movements are "unsuccessful," "useless," and have no direct or specifically stated effect upon learning. The movements that are supposed to be "accidental" in plunging the plane are, it must be admitted, as undirected as the so-called "useless" and the "unsuccessful" movements, and no specific difference can be said to exist in these movements. They must be regarded as practically the same in any scheme of "trial and error" that expects the appearance of the "successful" movement. The records of the first two rats presented do not admit the existence of "useless" movements or the "accidental" character of the first solution of the inclined-plane problem, but the "accidental" character of the first solution may, uncritically, be judged to be present in the records of the other rats. However, if the movements of these last rats are "accidental," when and how do they become "non-accidental," or "useful," or "successful"? Evidently definite changes must take place in some rats before movements can at all be effectively produced.

Whatever may be the interpretation of the early trials of learning in the inclined-plane problem, there are features in the solving or in the learning of other problems that are comparable to it. In many instances in solving the latch-box problem, several daily trials of 30 minutes each were required before the problem was solved. This prolongation of the time in rats for the solution of the problem solved occurred when reflex excitability was low or hyponormal; with the inclined-plane problem, which is invariably solved, there exists, when reflex excitability is hyponormal, a prolongation of the time when effective responses can be made to facilitate directive integration, and not for the solution of the problem, as is the case with the latch-box problem. Different situations in the two problems, one at the time of solving the problem, and the other in the early

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learning of the problem, require slow development of interaction of body parts for the production of effective movement. Thus the effect of the production of numerous movements upon learning is frequently observed, and these movements are not made before the solving of the latch-box problem, so that the "successful" movement can appear, or in the inclined-plane problem so that eventually one movement may become "successful."

This gradual development of interaction of body parts for the production of effective movements in rats when reflex excitability is low or hyponormal, is indicated by the observations of the behavior of rats. Usually in the first few trials movements are chiefly confined to the problem box. In a few additional trials progression from different positions under the hood covering the problem box is increasingly in the direction of the plane, and at times, it can be seen that vision aids direct progression to it. This behavior occurs more often in some trials than in others, and is always indicative of important changes that are to follow. These repeated advances to the plane are shown in tables 13 and 15 by increases in time required to plunge the plane, but are not to be found in tables 11 and 12 where the reflex extensor thrusts of the rats are stated as functionally fairly well developed and reflex excitability is manifest. These increases appear in the records of rats in which either the reflex extensor thrusts are undeveloped or when reflex excitability is hyponormal. In some instances when excitability is hyponormal, advances to the plane are so frequent that solution of the problem in one or two successive trials does not take place. This is shown in table 15. Even when the problem remains unsolved, these numerous movements or advances to the plane, produce important changes in learning. In the next trial or two, improvements in all responses follow, and either movements are noticeably more directive or, one part of directive integration, progression to the plane or to the door after the plane is plunged, is facilitated. Sometimes with the occurrence of numerous movements and an increase in time, there takes place a decrease in the number of movements, and in the time,

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before the plane is plunged, and other increases in the number of movements and in time appear before there occurs an improvement in the effectiveness of responses leading to the appearance of directive integration.

These repeated advances to the plane with an increase in time to plunge it, not only help to facilitate direct progression first to the plane, but also do so first to the door before directive integration is fully established. When reflex excitability is manifest only after the plane is plunged, increases in the number of advances and in the time to plunge the plane lead to the effective performance of movements, and to the facilitation of direct progression first to the door and not to the plane. This same effective performance of movements leading to direct progression to the door of the problem occurs after the problem remains for one or two trials unsolved. The effect produced by numerous movements in this last instance is not so immediate, but is noticeable in a trial or two after the problem is not solved. On the other hand, when reflex excitability is manifest before the plane is plunged, and even when an increase in excitability is again manifest after the plane is plunged, then the cumulative effect of all movements leads to the facilitation of direct progression first to the plane and not to the door. With early manifestation of reflex excitability, effective movements appear at any trial the moment a rat is admitted to the problem, whereas, when excitability must be aroused, by the plane going down, effective movements appear at the end of a trial.

Not alone is directive integration facilitated with repeated advances to the plane, but these advances are made to one side of it and a constant method to plunge the plane is used. When progression is to one side of the plane and one method to plunge it occurs, then that side of the plane actually determines the direction of progression from the entrance box to the plane. This advance to one side of the plane is usually facilitated whether direct progression is facilitated first to the plane, or first to the door; but at times when direct progression to the door is not readily facilitated, progression may be to the right or to the left of the plane for many trials. Usually, then a

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variety of methods to plunge the plane occur until responses become more directive and effective.

The facilitation of direct progression in one common path from the entrance box to the plane is a slow process, even in rats with fairly well developed reflex thrusts. It was shown in table 11, that unobservable decreases in the extensor tone of the fore limbs were supposed to account for the variations in direction to the plane, and this view can now be supported by observations of other rats. When the reflex extensor thrusts are asymmetrically developed, the direction of progression to the plane is to a great extent conditioned by the stronger extensor tone existing in the two ipsilateral fore thrusts when a rat's head is bent to the right or to the left of the midline of the body. In many cases, however, the direction to the plane is apparently conditioned by the position of the operator. When, for instance, the operator stands to one side of the problem, progression to the plane takes place usually on the side of the problem more distant to the operator; that is, if the position of the operator is constantly to the left of the problem, progression to the plane will generally be to the right of the problem to the plane. And when the operator is directly in back of the problem, progression is then conditioned by the development of the rat's extensor thrusts and the path so determined is illustrated in the diagrams figure 5, *A*, *B*, and *C*.

In rats in which a right ipsilateral fore reflex thrust exists or in which the extensor tone of the right is stronger than that of the left, progression is to the left of the problem as is traced in diagram *A*. The reverse is the case when a predominant left ipsilateral fore thrust is evoked, for progression is as in diagram *B*, to the right of the problem. Certain exceptions to this rule must be made, for some times progression to the plane, when the extensor tone of the left ipsilateral fore thrust is stronger than the right, is to the left of the plane, instead of to the right. This variation is due to the right fore thrust or neural center of the cord, influencing the left thrust or center of the cord; for, during learning, the extensor tone of the left ipsilateral fore thrust may become stronger and progression is no longer direct,

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but is diverted as is shown in diagram *C*. Progression may be then a short distance to the right or even to the plane, reversed and continued to the left of the problem. In one or two cases in rats in which progression was to the right of the problem and in which the left ipsilateral fore thrust had a stronger extensor tone than the right ipsilateral fore thrust in the beginning of learning, the extensor tone of the left ipsilateral thrust decreased and progression continued to the left of the problem to the plane. Very seldom is it the case that progression continues to the right of the problem when a left ispilateral fore thrust has a stronger

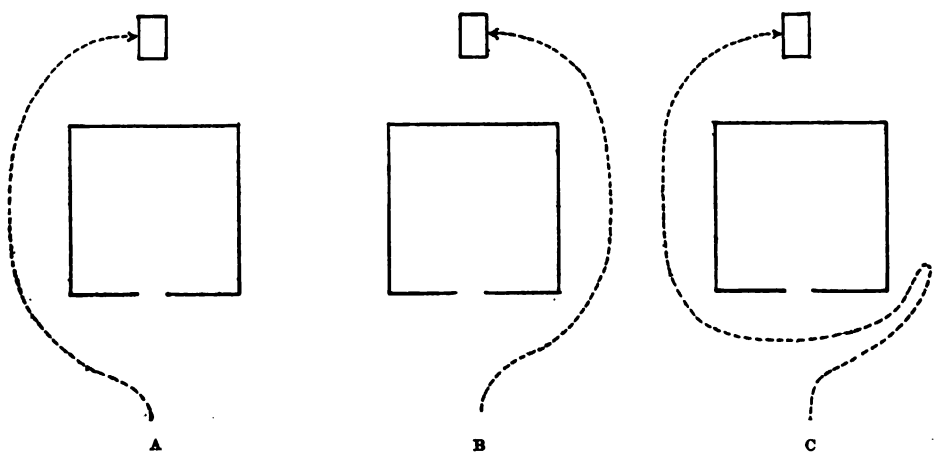

FIG. 5. DIRECTIONS TO THE PLANE

extensor tone than the right ipsilateral fore thrust. In some instances when no fore ipsilateral reflex thrust could be evoked, progression was then to the right or to the left of the problem. The left or right center of the cord then may predominate, for, if progression is to the left, the appearance of a weak left ipsilateral fore thrust will condition diverting of the direction of progression to the right; and if a right ipsilateral fore thrust appears, progression is usually diverted from the right of the problem to the left and continues thereafter to the left. From the results obtained from these observations, there always

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appears to be a greater tendency of the right ipsilateral fore thrust to gain an ascendancy over the left fore thrust. This statement will again be considered when investigating learning in the maze problem.

When direct progression in one common path to the plane is facilitated, plunging the plane must be accomplished, and this last act is the most difficult to perform. It was observed, in learning the latch-box problem, posturing to produce the neck reflex could not readily be facilitated, and again it is noted, that posturing and the production of the extensor thrust is as difficult as posturing under the latch, if not more difficult. In both instances posturing is dependent upon the degree of the development of reflex mechanisms. There exist therefore, similar difficulties which give rise in a way to similar behavior, some phases of which are more pronounced in the inclined-plane than in the latch-box problem. When the reflex thrusts in rats are undeveloped, the repeated advances to the plane indicate that posture is impossible and consequently methods to plunge the plane, other than the use of the extensor thrust appear, and these other methods are less efficient and even more difficult to use than the extensor thrust. In the case of the latch-box problem, where some form of posture is necessary to plunge the plane, imperfect responses before the latch occur and only one other method, the use of the teeth is employed. The repeated advances to the plane without plunging it are equivalent to the disruptions of directive integration in the latch-box problem; the use of methods to plunge the plane other than the extensor thrust is similar to the use of the teeth to raise the latch instead of the neck reflex movement. These conditions arise because posture is difficult to attain when the reflex mechanisms of the rat are undeveloped.

Again, as in the case of the latch-box problem, fluctuations in the reflex extensor thrusts often increase the difficulty to facilitate posture, and the continued use of one method. This behavior results in the production of imperfect responses. The records in tables 11 and 12 show that when only one method, the extensor thrust, is used, then fluctuations in these thrusts

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produce imperfect responses before the plane. Then, with or without a change in the position of the rat, one or more ineffective thrusts are produced before an effective one plunges the plane. Progression from, and a return to the plane may occur before an effective thrust plunges the plane. These ineffective extensor thrusts occur not only before directive integration is facilitated, but also afterward. On the other hand, when the reflex thrusts are undeveloped, or an unequal antagonistic action of extension and flexion of the limbs exist, fluctuations in the thrusts condition a similar performance, but many advances to the plane usually occur. In addition, it will be noted in other tables that fluctuations in the extensor thrusts will condition the use of another method not commonly used. In the latch-box problem the fluctuations in the thrusts have been noted to occasion the use of the teeth to raise the latch, when previously the neck reflex movement was employed; and in the inclined-plane problem a change in method results from similar fluctuations. Usually a change in method in the inclined-plane problem comes about by the performance of an ineffective method commonly used, or after the occurrence of an imperfect response. It has been noted, that when the extensor thrusts are fairly well developed, a change in method does not follow the ineffective performance of the extensor thrust to plunge the plane. Sometimes there appears to be no difference between the production of an imperfect response and the use of other methods than that of the extensor thrust.

The substitution of one method for another does not occur with great frequency, but when for instance the modified thrust is used for a time, another method for a trial or two may be used. Often when running and stepping on the plane does not plunge it, or the plane is bolted, progression is reversed, retracing of steps to the plane occurs, and the plane is plunged not from the usual side, but from the other side by the modified extensor thrust. Or, if this last method proves again to be ineffective, the usual running and stepping on the plane in the customary direction is once more resorted to. This change in method is often indicated in tables 13, 14, and 15, when fluctuations in the extensor thrusts occurred.

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Every movement other than the customary one, results from a change in the functional condition of the animal. Some objection may arise to this statement, when such complex behavior is observed as the substitution of one method for another when the first is ineffective. It may be contended that the rat "does something," and that the movements of the rat are not strictly determined by the functional condition of the organism. But this substitution of movements is gradually facilitated, and does not appear suddenly. A return to the plane and the use of another method to plunge the plane occurs only in later trials in learning. It is easily observed to be determined by the absence of sensory excitations which are usually aroused in the organism when the plane is plunged, and which condition direct progression to the opened door. Progression, when the plane is plunged, is ended on the other side of the plane and a return movement is facilitated, or progression around the problem box takes place once or twice before the plane is plunged. Sometimes progression is continued ineffectively as far as the door and not around the problem. At such times and when progression is ended on the other side of the plane, wandering movements in any direction appear. Effective posture for progression in a definite direction does not take place, and eventually a return movement is facilitated to the plane. But the plane is not immediately plunged, for a facilitation of progression and the use of another method must take place. One ineffective method disturbs the established physiological sequence of movements and another sequence is facilitated. Other movements compensate for an ineffective sequence of movements.

Another physiological condition besides the existing development of the extensor thrusts influences the effective performance of movements. Fluctuations in reflex excitability also appear as well as fluctuations in the extensor thrusts. When fluctuations appear in reflex excitability, movements may be more rapidly, more effectively performed with an increase in excitability, and more slowly and less effectively performed when it becomes hyponormal. A change in a rat's movements is more noticeable with a decrease than with an increase in reflex excit-

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ability. When it is hyponormal, progression from the entrance box to the plane may be slightly diverted or wandering progression may take place in any direction. Sometimes climbing and wandering movements return characteristic of the early trials of learning. Similar behavior was observed when investigating learning in the latch-box problem, and as was noted in table 8, progression to the latch was ineffectively performed. With the inclined-plane problem, identical ineffective progression appeared, and this is indicated in table 15. With the rat whose record is presented in this table, frequently effective responses and partly ineffective posturing before the plane appeared for the production of the modified extensor thrust.

Such consideration of the physiological conditions as determines the effectiveness of every movement and every method, is significant in the understanding of learning of every problem. Compensation in the use of other methods is very inadequate when measured with the results obtained—the number of perfect consecutive records. The number of these records indicates the effectiveness of the extensor thrust as compared with other methods used in learning the inclined-plane problem. The ineffectiveness of the use of other methods is not alone measured by the number of perfect records, but by the use of other localized parts of the rat's body to plunge the plane when the extensor thrust cannot be produced. Physiological compensation appears as a result of the undeveloped condition of the organism. When the modified extensor thrust is used, the weight of the fore part of the body is added to the force exerted by the extensor thrusts to bring the plane down. When the plane is ascended, compensation is attained by the use of two localized parts, the fore and the hind limbs with the weight of the body. In a similar manner, the weight of the body is used when in running either the fore or the hind limbs strike the plane and the plane is plunged. And this compensation in the use of other parts occurs with the same tendency as was observed to take place in learning the latch-box problem, there takes place a reduction in extensor tone of the rat's limbs to restore symmetry to both sides of the body.

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These observations point inevitably to the fact that learning the inclined-plane problem is alone effective when the extensor thrust is used. Other methods used are ineffective in proportion to the functional development of the rat. At times, consecutive perfect records are produced with several imperfect records intervening. More frequently such consecutive records are made before the end of the sixtieth trial, sometimes much before this trial, and thereafter none appear. In a few instances, when in the early trials consecutive perfect records are produced, none are made even after 125 trials are given. A *repetition* of the number of trials does not then improve learning. Often only isolated perfect records are produced. In such cases directive integration is facilitated, but the plunging of the plane the first time progression occurs to it, is impossible. The ineffective responses produced are not conditioned by inadequate excitations in the senses, but by the undeveloped organism.

From the records obtained, from the behavior of the rat before the plane, little can be said of the significant part played by the senses. Knowing the functional condition of the organism, it would be needless to over estimate its importance in learning as has generally been done. When considering the importance of the reflex mechanism and the changes in behavior brought about by their fluctuation, rather less importance must be attributed to the part played by the senses. Responses, whether adequate or inadequate, are produced by excitations in the external senses, and an exclusive rôle in learning to be mainly attributed to the senses is an exaggerated estimate of their importance.

For several reasons the importance of the senses in learning must be reconsidered. The use of other methods than the extensor thrust is not the result of sensory discrimination, nor the product of their excitations, as must be supposed if the freedom of choice of a method is left to the rat. By the very few perfect records produced when the reflex mechanism of the rat is undeveloped, it is seen that the external senses cannot improve upon a method. Fluctuations in the reflex extensor thrusts have alone been observed to improve, temporarily, a

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method used. Sensory control of movements has never been observed to be present. Both when the reflex mechanisms are developed and undeveloped, progression once started by excitations in the external senses, continues without further excitations from these senses.

Direct evidence is not wanting to show that the external senses, in a definite way, direct progression and produce responses that solve a problem. There is evidence that stimuli from different parts of the environment spread excitations in the senses, increase the extensor tone of muscles for posture, for progression, and for the production of definite movements—such as the extensor thrust to plunge the plane. Without the facilitation of posture of some kind, the external senses cannot, it appears, direct movements nor produce them. This increase in extensor tone for posturing is facilitated by the development of an interaction with other reflex body parts, such as that for the production of reflex excitability and possibly other reflex parts. In the inclined-plane problem, this development of interaction of body parts for the attainment of posture, is observable when the cumulative effect produced by all movements, in particular when the plane is plunged, results in the improvement of responses. At this time, the plunging of the plane produces rapid responses, indicating a spread of excitations from the internal and external senses. Rapid descent from the plane, turning around, using the olfactory and visual senses show that many senses and other body parts interact. Such an interaction is the beginning of the facilitation of posture for directive integration and the performance of movements to plunge the plane. In some instances rapid descent and progression from the plane is stopped by the noise of the door opening. When progression stops, the body is crouched and this position must increase the interaction of body parts. The most that sense functioning can be said to accomplish is to hasten the development of interaction of parts, and this is again hastened when reflex excitability is hypernormal, and retarded when it is hyponormal. Observations once more substantiate the view that the sense organs function in no such exclusive manner in order

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to establish "sensory association" or "sensory motor connections."

The development of interaction in learning the inclined-plane problem may be graphically represented by curves. Three curves delineating this development are presented in curves I, II, and III and IV. Points on the abscissa represent trials, and those on the ordinate, the reaction time for a trial. A length of 2 mm. on the abscissa represents one trial, and 2 mm. on the ordinate 2 seconds. Curve I, graphically produces the records of a rat which possessed fairly well developed reflex thrusts; curve II a rat with these thrusts undeveloped; and curve III, one with both undeveloped reflex thrusts and mechanism for the production of reflex excitability.

A description of these curves must not be a general one, for now there is need for greater details. If learning is the development of interaction of reflex body parts, the three curves must graphically represent the results obtained from learning with rats with these parts fairly well developed, and with others having these parts undeveloped. The general outline of each curve must be different in accordance with the degree of development of interaction of parts for learning. Every altitudinal point, the upslopes, represent the things that prevent a ready development of interaction, and signify an increase in time to plunge the plane; and every descending point, the down-slopes, in the direction of the abscissa, indicate that interaction is becoming effective and there is a decrease in time to plunge the plane. A decrease in the height of the majority of the up-slopes, from the first to the last trial, shows that the development of interaction for learning is progressive.

Curve I is constructed from the records presented in table 11, which reveal in a rat the reflex extensor thrusts fairly well developed and reflex excitability manifest. In the first trial, the reaction time is short and the first point in the curve is close to the abscissa and the second is still closer to this line. In fact the second trial is perfect. There exists therefore, for these two trials not great altitudinal points, but in the next trials these points greatly increase in height with equally great descending

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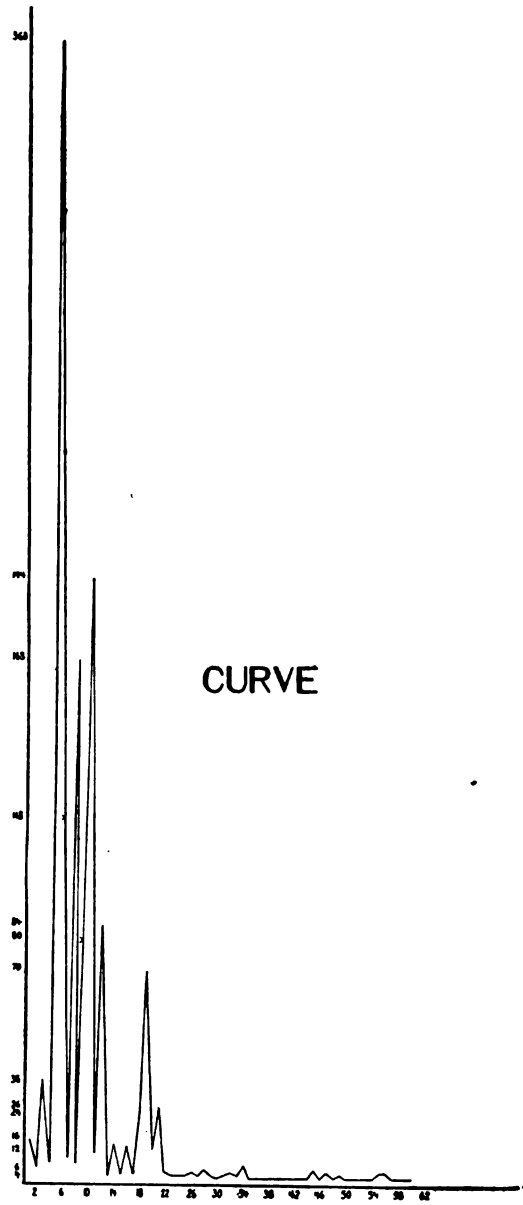

CURVE I

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points. This makes the curve very irregular in outline. The descending points represent the fact that directive integration is difficult to establish because of fluctuations in the extensor thrusts. Any descending point not representing a perfect record, indicates that imperfect responses or ineffective extensor thrusts are made before the plane without a change in the position of the rat. When a high altitudinal point is reached and directive integration is facilitated for any trial, ineffective responses are made before the plane, and a change in position—that is progression from the plane—and a return later to the plane, occur. A very high altitudinal point may also mean repeated advances to the plane took place. When directive integration is facilitated, and interaction of body parts is developed, then perfect records for several consecutive trials are produced. Then greater and lesser upslopes and downslopes appear, and they can be said to be due to slight decreases in reflex tone or to other changes in the rat. When marked fluctuations in the coördination of extension and flexion of the limbs occur, then the imperfect responses that are produced increase the altitudinal points, that is, a great upslope appears.

Curve II, was constructed from a rat's record presented in table 13. The rat whose record is given in this table possessed undeveloped reflex thrusts, and manifested in the early trials of learning hyponormal reflex excitability. There exists on this curve from the first and the second trials, high altitudinal points and as marked descending points in the next few trials. When directive integration is facilitated and reflex excitability manifest, points in the curve are closer to the abscissa. At this time the method used to plunge the plane—that is running and stepping on the plane—has become fairly constant. Of the 60 trials given only 3 perfect trials are recorded. All upslopes in this curve indicate that the method used was at times more effective, yet rarely leading to the production of perfect trials. When the results obtained are measured with those of preceding rats, it is clear that only partial interaction, because of the existence of undeveloped reflex mechanisms, is possible. The delineation of this curve reveals this inadequate interaction of parts for learning.

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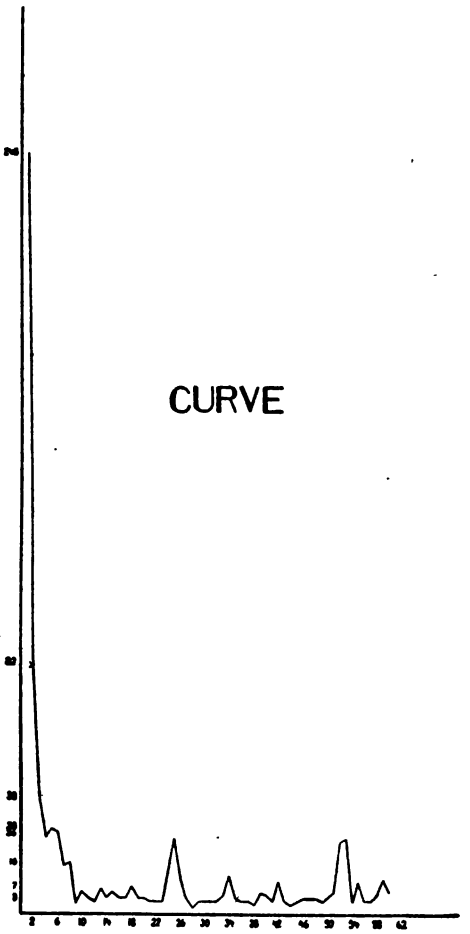
Greater altitudinal points are found in curve III, constructed from a rat's record given in table 15. The reflex thrusts in this rat are also undeveloped and reflex excitability is at times decidedly hyponormal. After the first trial, greater altitudinal



CURVE II

and greater descending points produce with 4 failures to solve the problem in 30 minutes a very irregular curve. High altitudinal points in the early trials are due to fluctuations in the reflex thrusts with changes in the method to plunge the plane,

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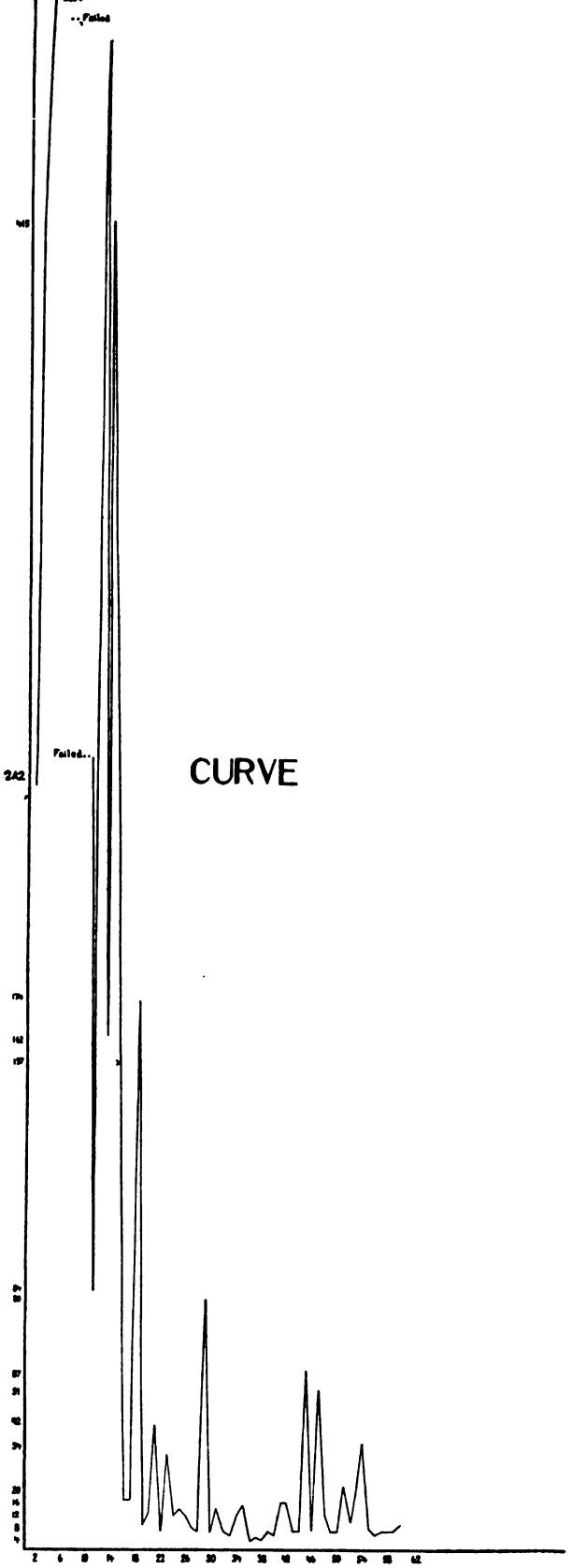

CURVE

CURVE III

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and low points are due to more efficient movements. Upslopes, with increases in time before the problem is solved, represent advances to the plane with wandering progression, downslopes, with decreases in time, show the occurrence of fewer of these movements, and a greater effectiveness in the method employed. Effective responses produced 2 perfect records. Occasionally fluctuations in reflex excitability decrease activity and increase altitudinal points. When reflex excitability is constant, generally high altitudinal points are not great. Greater altitudinal points, representing the presence of hyponormal reflex excitability, and increases in time, are presented in curve IV. This curve shows no great differences from the other 3 curves but reveals more pronounced changes in the organism when learning. From the twenty-third to the thirty-third trial perfect records were made, but none thereafter. The modified extensor thrust was used.

These curves represent the degree of effectiveness of every movement made by a rat, and this effectiveness is dependent upon the degree of the development of interaction permissible, when, in two rats the functional condition of one organism is fairly, and when in two, poorly developed. These curves show a gradual change in the physiological condition of the organism. Their interpretation rests upon physiological changes, not upon a mathematical calculation of any kind.

By comparison of the results obtained when investigating learning in the latch-box and in the inclined-plane problem, it is evident that the rat's organized mechanism of reflexes does not work the same in two dissimilar situations. Accordingly, there appears a diversity in the results obtained from the two problems. The situation presented by the latch-box problem is without question best suited for the rat's reflex mechanism, but even here there are limitations, for when rats manifest hyponormal reflex excitability, posturing to produce the neck reflex is at all times impossible. On the contrary, posturing is not necessary in the inclined-plane problem to plunge the plane. But, proportionally greater is learning in the latch-box, than in the inclined-plane problem. Only in two cases did the latch-

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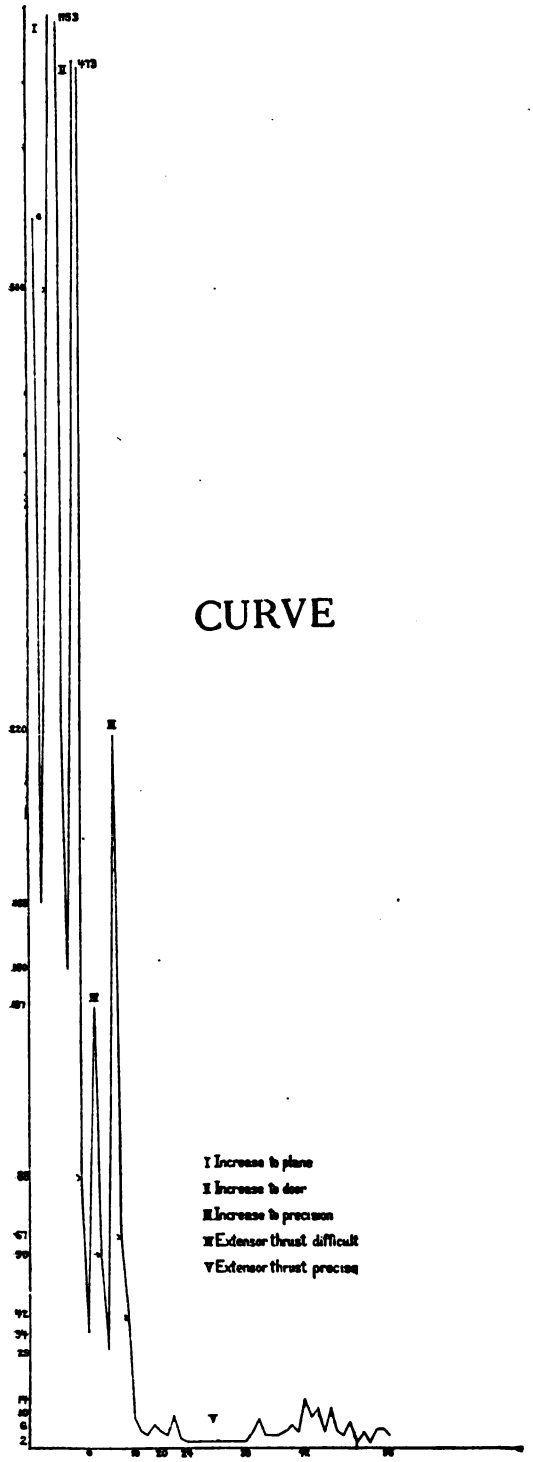
I Increase to plane
I Increase to deer
II Increase to precision
III Extensor thrust difficult
V Extensor thrust precise

CURVE

CURVE IV

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box remain unlearned, whereas in only two rats, judging from the number of consecutive perfect responses made, was the inclined-plane actually learned. This is because the effectiveness of posturing and the production of the neck reflex to raise the latch is greater than the effectiveness of posturing and the production of the extensor thrust or any other method that plunges the plane. When the latch is raised by the neck reflex, the head of the rat is moved upward which increases the extensor tone of both fore limbs, and decreases the extensor tone of the reflexes of the hind limbs. The opposite effect is produced when the head is lowered, the fore limbs are flexed and the extensor tone of the hind limbs is increased. Now it is the last condition that exists when posture is usually attained before the plane, the head tends to be lowered when posturing, and no increase in the extensor tone in the fore limbs is given, so that the extensor thrust to plunge the plane can be produced. The head is, however, raised when the reflex thrusts of rats are well developed, and this is accomplished by crouching the body. In the two rats in which the extensor thrusts were exclusively used, the body was crouched with the head raised some time before the plane was reached, and when the plane was approached, the head was raised still more and the extensor thrusts of both or of one limb plunged the plane. Thus, two rather similar postures are demanded, one to produce the neck reflex and the other the extensor thrust; but to produce the extensor thrust requires a better developed reflex mechanism. When the reflexes are undeveloped, progression is rapid to the plane and other methods are used to plunge the plane. The development of interaction of body parts to attain posture to perform fundamental movements is then impossible.

In the investigation of learning the inclined-plane problem, there is no physiological necessity for the acceptance of any antithetical division of movements and the retention of the "successful" or the "congruous" movements, for in the inclined-plane problem, it is difficult to say when such movements are produced. Pleasure cannot be said to produce fixation of movements in learning; for the supporters of "pleasure-pain" would

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find it difficult to account for the fact that direct progression to the plane occurs in some rats, before it does to the opened door, after the plane is plunged. It would be expected then, that the performance of the plunging of the plane gives "pleasure" and not progression to the food. If this is so, then on the other hand, it could not be said to account for the fact that sometimes the reverse takes place and progression occurs, first to the door and not to the plane. Here reward would be assumed to fixate direct progression to the food. The effects usually said to be produced by the arousal of pleasure can not be operative in two directions. Moreover, pleasure can not account for the facilitation of both parts of directive integration simultaneously, when the reflex mechanisms of the rat are developed. Much less can a mechanical repetition of a movement explain how at different trials progression to or from the plane takes place.

No problem shows, as does the inclined plane problem, how little reliance can be placed upon repetition in learning. For after 60 trials only a few perfect records are sometimes made, and a repetition of trials does not increase the number of such records. When several consecutive perfect records are made, additional trials ought to produce a continuation of these records, but as the tables and the learning curve IV show that after a series of perfect records produced much before the sixtieth trial, no additional records of the kind are made, and the results are very poor. Apparently a repetition of trials has no more physiological effect than the taking of food repeatedly has for the growth and the development of an organism. More fundamental considerations are required for the understanding of either learning or of growth. Moreover, the repetition of trials with accompanying sensory excitations have not that predominant effect that is attributed to them when it is said that they lead to modifications of the nervous system, such as increasing the permeability of the so-called synapses or, the selecting of neural arcs. Reintegration of the nervous system is not a fundamental consideration in learning, for the inherent integrated reflex mechanisms are not improved by learning. In

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such instances when rapid progression occurs from the entrance box over the plane to the opened door, and in another instance when progression is to the plane, the plane plunged with the extensor thrust and progression continued to the opened door, and all gradations exist between these two extreme methods, it is difficult to believe that definite modifications in the nervous system alone account for the different ways in which the plane is plunged. These various ways exist because of the inherent functional condition of many body parts interacting in learning.

Because of the dependency of the movements to plunge the plane upon this functional condition of the rat, it can not be supposed that the rat plans its own actions, using alternative methods when the most adequate method to plunge the plane, the extensor thrust, is at all difficult to produce. All rats come to use the method of plunging the plane in accordance with the development of the reflex thrusts. There is no evidence that the rat tries one way then another, until the "successful" movements appear, or that the behavior of the rat is ascribable to previous experience; that is, there does not exist a correlation of past experiences with subsequent behavior, unless there is meant by this a progressive development of physiological interaction of body parts to facilitate the production of effective movements which the problem demands. What is observable is compulsory behavior, and probably much of the so-called "intelligent" behavior will eventually be regarded in the same light. Consequently, it does not seem to be scientific to say that a rat "does something" when different responses are made to one situation, and when different methods of plunging the plane are dependent upon the development of fundamental reflexes; or when different responses are made to two different situations, the latch-box and the inclined plane problems, which demand different fundamental movements, the neck reflex and the extensor thrust; or when different methods are used in the same situation when fluctuations occur in the extensor thrusts.

The effective adjustment in one rat to the inclined-plane problem in the second trial and the same adjustment in two rats in the second and third trials to the latch-box problem,

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indicates a high degree of the development of the reflex mechanisms in these rats. From these results it appears that the usual distinction between a "habit" and an "instinct" is an artificial one. The only distinction noted between a movement, as the extensor thrust to plunge the plane, and a similar movement of the extensor thrust to push light objects about, is that in the first case an exaggerated performance of this thrust of the fore limbs is used. In each case progression is inherently conjoined with the extensor thrust, and for plunging of the plane an exaggerated performance of both movements is demanded. In a great many rats this exaggerated performance of these movements can not be produced, and for them the limitations to effective adjustment to the environment of the problem are observed to be present.

(To be continued)

REFERENCES

(1) Carr, H.: Principles of selection in animal learning. Psych. Rev., xxi, 1914.
(2) Watson, J. B.: Behavior, An Introd. to Comp. Psych., 1914.
(3) Holmes, S. J.: Studies in Animal Behavior, 1916.
(4) Loeb, J.: Forced Movements, Tropism, and Animal Conduct, 1918.
(5) Carlson, A. J.: The Control of Hunger in Health and Disease, 1916.
(6) Carlson, A. J.: Amer. Jr. Physiol., xxi, 1913.
(7) Elliott, T.: Br. Jr. Physiol., xliv, 1912.
(8) Cannon, W. B.: Bodily Changes in Pain, Hunger, Fear, and Rage, 1920.
(9) Carlson, A. J.: Ibid., 1916.
(10) Magnus and de Kleijn: Pfluger's Arch., 145, S. 455, 1912.
(11) Magnus and de Kleijn: Pfluger's Arch., 145, S. 178, 1913; 159, S. 163, 1913.
(12) Sherrington, C. S.: Brain, xxxviii, 1915.
(13) Beritoff, J. S.: Jr. Exper. Physiol., ix, 1916.
(14) Jennings, H. S.: Behavior of the Lower Organisms, 1906.
(15) Beritoff, J. S.: Ibid., ix, 1916.
(16) Beritoff, J. S.: Ibid., ix, 1916.
(17) Sherrington, C. S.: Proc. Roy. Soc., Series B, No. 80, 1908.
(18) Gruber, C. M.: Am. Jr. Physiol., xliii, 1917.
(19) Rogers, J., Cooms, H., Rahe, J.: Am. Jr. Physiol., xlv, 1918.
(20) Pratt, F. H.: Am. Jr. Physiol., xliv, 1917.
(21) Beritoff, J. S.: Ibid., ix, 1916.

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